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Acoustic Dimensions of Functor Comprehension in Broca's Aphasia

by

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A THESIS
SUBMITTED TO THE FACULTY OF GRADUATE STUDIES AND RESEARCH
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language is better analyzed by aphasia than by linguists.
—Jason W. Brown

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The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies and Research, for acceptance, a thesis entitled Acoustic Dimensions of Functor Comprehension in Broca's Aphasia submitted by Randy Allen Harris in partial fulfilment of the requirements for the degree of Master of Science in Psycholinguistics.

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Date 15-~~July~~ August 1985
lms

DEDICATION

This thesis is dedicated to the memory of Sephas Carey Harris, who lost his language, his strength, and his life, in a relentless series of strokes, and to his loving wife Gladys, who was his voice, his strength, and his reason to live. She remains an inspiration to everyone who knows her.

ABSTRACT

This thesis is organized around Heilman and Scholes' (1976) elegant test of functor comprehension in aphasia, with the addition of a control on the acoustic dimensions of the key function word. Nine Broca patients heard four sentence pairs differing only in the postverb placement of *the* (and the argument structure cued by that placement), as illustrated by the following two sentences:

> The man showed her the baby pictures.
> The man showed her baby the pictures.

The task was a forced choice between four line drawings: one which depicted the correct action, one which depicted the action of the other member in the pair, and two which contained depictions of different figures and actions altogether. Each of the eight sentences was played in two conditions: one with NORMAL intonation, and another with an acoustically more distinct SALIENT *the* token in the postverb location.

Both of Heilman and Scholes' principal results were successfully replicated: the aphasic population was significantly less reliable overall than normals, and the difference between correct responses and function errors (choosing the depiction of the paired sentence) was not significantly greater than chance for the NORMAL sentences. Both of these results indicate that Broca patients have a peculiar deficit for some function words. However, a SALIENT effect was also discovered. The subjects were significantly more reliable with the acoustically boosted stimuli. This finding is used to suggest that there is a performance component to Broca syndrome functor difficulties, and therefore that proposals of lexical or syntactic or phonological incompetence are overzealous. A tentative explanation of the results is offered in terms of short term memory deficiencies.

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1. Introduction

The brain is a very mysterious organ and its functioning is far from clearly understood, but immeasurably more is known about it now than at any time in the past. To take a radical example, Aristotle saw the brain rather as a spongy radiator – a cooling device where blood, exited by muscular activity, could dissipate its heat. Herophilus thought it generated 'animal spirits', his medium of a hydraulic theory of muscle control. Th. Bartholinus, some centuries later, held it to be packing material, installed to "protect the cerebral vessels against the danger of violent movement, especially during the full moon when the brain swells in the skull" (Schiller [1979:168]). While some of us act now and again in a manner suggesting the accuracy of these proposals, in hindsight their only charm lies in absurdity. But they are not unreasonable hypotheses when evidence is drawn exclusively from autopsy of neurologically intact cadavers. Brains look a good deal more like radiators or packing material than "the most elegantly organized bundles of matter in the universe" (Calvin [1983:62]).

Fortunately for science, the misfortunes of a great many individuals have led to more soundly-based theories of the brain, and growing insight into the elegance of its organization. Cerebral pathology has afforded an irregular and winding, but functional, roadway from radiators to receptive fields. When damage to a localized region of the brain results consistently in a disruption of abilities present in people without such damage – vision, motion, calculation, language use – then it follows that the brain mediates the ability. It also follows that the specific site of damage mediates the ability. How the mediation occurs does not follow. For instance, the damaged region might be a knowledge store, or an access channel to a knowledge store, or it might subserve a largely independent mechanism incidentally employed in the access or utilization of knowledge. Or the damage might simply be running an interference pattern, diverting energy from processes necessary to the disrupted ability. Or some complex of these posibilities might be responsible for the impairment. None of this is clear from the injury-dysfunction correlation alone. But there are methods of getting at these questions, and an experiment with such a goal is at the heart of this thesis.

Neither the experiment, nor the specific question addressed, is difficult to summarize: the acoustic patterns of a small set of speech strings are digitally altered so that certain normally reduced elements (functors) are increased in duration, intensity, and stability of fundamental frequency (f_0); if reduced stress is a significant factor in the difficulty Broca's aphasics have comprehending function words during sentence processing, then they should demonstrate improved comprehension of those strings. The motivations and the significance of the experiment are not nearly so straightforward.

1.1 The Issues

1.1.1 Site and Syndrome

Aphasia is a language impairment consequent to neural insult, and location of insult makes a difference. Though there has been considerable debate between equipotential and modular theorists about site-specific deficits (see, e.g., Finger and Stein [1982:13-47]), this much is obvious. Damage to the peripheral nervous system, for instance, or the central nervous system below the medulla, does not result in prolonged linguistic interference. There is some interference, and this interference does say something interesting about the mechanisms of language use. But the problem is short term, and probably due to some incidental diversion of energy. There is no aphasia. Similarly, damage to the right hemisphere, even if quite severe, has minimal effects on language in nearly all cases. Damage to fairly large tracts of the left frontal lobe also leaves linguistic ability pretty much alone. Insult to the visual cortex, or to the upper stretches of the motor strip, does not directly affect language use. On the other hand, damage to the convolution adjacent and immediately anterior to the fissure of Sylvius, or to the posterior temporal operculum, results in aphasia very frequently. Damage to the bridge between these two areas, the arcuate fasciculus, results in aphasia. Recently there has even been evidence that thalamic lesions, subcortical, measureably interfere with linguistic ability (Mateer and Ojemann [1983]). Clearly the issue here is not localization, but relative localization.

The equipotentialist position is that the brain has no functional divisions, that all cortical material, *en bloc*, serves all human cognition. The modularists claim all interesting functions are locally distinct. They delight in diagrams, mapping the brain section by section, with the ultimate goal of labelling each individual neural circuit with the word or transformation or feature detector it subserves; their patron saint is Gall the Phrenologist. Patently, both sides are made of straw, and they have been fashioned by the groups most in need of straw theories, the opposition. Neither the tight pocketing of Gall nor the vagueries of equipotentialism are sensible, and sensible neuroscientists recognize this. Aside from random aberrations, common to any intellectual pursuit, radical equipotentialism exists only as a rhetorical device employed by theorists focusing on site-function correlation; and radical schematics of functional topography exist only as a rhetorical device employed by theorists with a healthy respect for the scatter and overlap of aphasic syndromes. One side claims language is too complex a phenomenon to be modular. The other side claims language is too systematic to be diffused throughout the brain. But neither side is stupid, and both sides have a wealth of support in aphasic data: bewildering diversity is there for the asking, as are over-riding patterns. More to the point, both diversity and patterning can be rather smoothly accommodated by either camp. Locationists can point out the manifold complications of aphasia data and marvel that recognizable trends of any sort emerge: if distinct shapes can be detected through the haze of so many confounding variables, they must be very distinct indeed. The equipotentialists, with numerous examples from sciences as diverse and pertinent as psycholinguistics and chemistry, can explain the patterns away as artifacts of the variables. Each side has a good deal of room to manoeuvre. The issue, again, is relative.

Nevertheless, there are patterns which emerge from the data, and this thesis takes the position that, theoretically if not clinically, they are better investigated than ignored. Like most such works it begins with two patterns, the "two most important and common forms of aphasia" (Kertesz [1982:43]) – Broca's aphasia and Wernicke's aphasia – though Kertesz' statement requires some qualifications he does not supply. In particular, it requires a modifying prepositional phrase on the order of 'in the linguistic literature'. The reason they are important and common is that they represent largely distinct clusters of symptoms, associated with damage to distinct cortical regions, and they get a lot of press. Even strong equipotential theorists, who would probably claim the disorders are distinct because they reflect different ends of a continuum, or of a vector, must concede that there is sufficient evidence to support at least this division. Convincing arguments exist that the distinction is valueless, even detrimental, for clinicians (e.g., Schuell [1965]), and it receives much less attention in the clinical literature. But there are probably convincing arguments that the physicist's distinction between mass and weight is not suited to structural engineers; that does not make the two properties one. In any event, it is the patterns, not the chaos, that allow linguists to form workable hypotheses about the psychological nature of language.

But the tenuous fabric that the patterns are imprinted on, the background of chaos, needs always to be kept in mind, and any extensive work in experimental aphasiology is obliged to include a list of riders and cautionary notes. The problem is not that others in the field, or in closely related disciplines, are unaware of how provisional most conclusions must be, but it is at once difficult to extrapolate beyond the highly constrained nature of the work, and discouraging to think its results can only bear on the small subject group tested. The reasonable procedure, then, is to establish a network of flags to identify areas where the ice is thinnest, and cross the lake with caution. It also seems reasonable to do the flagging at the outset, allowing readers the latitude to chose their own path, or to turn back. So, before offering a definition of the syndrome of concern and its primary foil, Broca's aphasia and Wernicke's aphasia, the troublesome nature of the data that support them must be addressed.

1.1.2 Complications of Aphasic Data

The most immediate problem, for theorists of either stripe, involves the matching of lesion sites. As many have observed, nature is a sloppy experimenter, at least in matters cognitive, and cortical damage to precisely the same region in two aphasia victims is very rare. Anatomy is largely the same, person to person, and it might be supposed that ruptures and blockages would occur in the same principal areas with reasonably high probability; that very similar damage would occur in large subgroups. Ultimately, a site-syndrome line of reasoning has to fall back on such a premise; but consider the variables. While gross anatomy is largely the same, there is much interesting variance in the details: height, eye, hair, and skin colour, facial bone pattern, sex, and even blood protein, differs person to person; some of the differences are inborn, some are environmental. Brains, and the probability of particular orders of brain damage, are subject to the same kinds of variability. To scratch only the surface: genetic factors (affecting, e.g., sex, development, handedness, and several classes of cerebral disorders), dietary habits (affecting, e.g., blood pressure and cholesterol count), and general lifestyle (middleweights having a different cortical make up than secretaries, alcoholics differing from teetotalers), all conspire to reduce the probability of precise damage match-ups to a much lower level than first blush would indicate.

And brains are malleable, dynamic organs. They learn, and hence reorganize. For example, the syndrome to be investigated below, Broca's aphasia, is defined in part as a symptom complex correlated with insult to the third prefrontal gyrus of the adult left hemisphere. Yet many features of this complex result from damage to tracts of the right hemisphere in children under six, and no language disorder is evident in many children with left third prefrontal gyrus injury. More dramatic examples are available, such as Smith's (1977) case of a linguistically (and intellectually) intact twenty-five year old who had undergone a left hemispherectomy at the age of five.[1] Obviously, at least one major reorganization of the language faculty (or access channels, or incidental processing mechanisms) can be charted as a function of age. The plasticity of young brains is phenomenal, and although there is no question that this plasticity decreases profoundly after young adulthood, it does not go away. Brains do not regenerate, but they do reorganize. They recover, and therefore, to brave a somewhat metaphysical image, it is not untoward to suggest that *minds* regenerate. Moreover, there is some evidence (from aphasiology) that mental reorganization is an ongoing process, which occurs even in the absence of brain damage (Brown [1977, 1979, 1982], Brown and Jaffe [1975], Obler *et al.* [1978]).

---

[1] Smith (1977) was presented at the fifth annual meeting of the International Neuropsychological Society, and has not to my knowledge been published. It is cited by Finger and Stein (1982:140f,152).

Brains differ, and they differ in manners not always matched easily to global properties like age, sex, or handedness. There have been, for instance, a few attested cases where damage to Broca's region in normal, adult, right-handed patients did not impair linguistic ability — including an instance of complete surgical "excision of Broca's area without persistent aphasia" (Zangwill [1975]). Types of damage, etiologies, also differ, and they have different consequents; all aphasia patients are not the victims of vascular rupture or blockage. Slow cortical damage (e.g., a tumour) allows the brain considerable time to reorganize.[2] Sudden damage (e.g., a trauma) allows no time at all for concurrent reorganization, though reorganization can and frequently does take place post-insult (c.f., Finger and Stein [1982]). Tumours also grow at different rates, depending on a multiplicity of poorly-understood factors, so two victims of neoplastic damage to the same general areas might display significantly different symptoms. Similarly, there are differences in sudden damage — trauma usually being more discrete, more focal, than hemorrhage or infarction.

---

[2] The patient Zangwill reports on underwent tumour removal, and an argument designed to neutralize Zangwill's counter-evidence to site-symptom correlation has been advanced to the effect that the tumour displaced the 'real' Broca's area, which then settled back in once the imposter gray matter was excised (see the discussion appended to Zangwill's paper). If indeed Zangwill's case study is counter-evidence, as opposed to an exception, a more profitable answer seems to lie in the reorganization principle.

A further confounding variable, or class of variables, is implicated in the difficulties of isolating linguistic symptoms from impairments of another order, from motor problems or other cognitive deficits. It is, for instance, by no means obvious that the right side hemiplegia which so frequently attends Broca victims should be divorced from their language disturbances. In fact, there is an interesting neurobiological theory that speech evolved from the sequencing abilities necessitated by stone throwing, using that capacity as the "foundation for the language cortex" (Calvin [1983a]). And of course this difficulty of partitioning language and muscle disorders compounds when the motor dysfunctions involve direct control of articulation (dysarthria) or indirect, motor-plan controls of speech (apraxia), and the impairment of ostensibly nonlinguistic cognitive abilities, like calculation or memory, is even more problematic. Quite simply, an aphasia victim with hemiplegia might have a qualitatively different disorder than an aphasia victim with unimpaired movement. An aphasic with calculation or memory difficulties might have a different disorder than a classically 'pure' aphasic. Or it may be, as suggested in the final chapter, that at least some classically pure aphasias strongly implicate capacities not exclusive to language — like short term memory.

These complications come in addition to the standard battery of subject variables that can skew or mask results in nonpathological psycholinguistic experimentation – socioeconomic background, number and type of languages spoken or understood, education, I.Q., developmental language or nonlanguage disorders (e.g., dyslexia or tinnitis), and a host of more subtle interferants (such as experimental attitude) – and all of these factors are somewhat more serious for pathological studies, since there is a much smaller population from which to choose. Often the experimenter is forced to work with subjects of widely different educational, social, or linguistic backgrounds, or even widely different pathological symptoms, because they are the only subjects available. Many of the experiments reported in the following chapter investigate ill-defined subject pools, carrying only the label "aphasics". Aphasia subtypes and severities are frequently not discussed, leaving the impression for an educated reader of a heterogeneity the experimenter prefers to ignore.

Still, there are controls. The same methodology available to responsible nonpathological psycholinguistic experimentation is available to responsible aphasia work. First, the population should be made as homogenous as possible, including such ostensibly unimportant factors as hemiplegia. Next, as many variables as possible should be statistically analyzed with respect to one another, to see if education or age or (premorbid) I.Q. or pathological symptoms, or some interaction(s), significantly accounts for any of the results' variance. Third, since neither of the first two are fully within the experimenter's control (information as to premorbid I.Q. is not often available, for example, and sample sizes are often skewed for variables like hemiplegia), more flagging should be done. Detailed subject descriptions should be included, to give the reader interpretive latitude. These controls are particularly important in a work of any scope, such as a thesis, and while statistical analysis of these variables was unfortunately not supported in my data set, every attempt has been made herein to regulate and flag all potential sources of variance. (Brookshire's [1983] very sensible guidelines have been adopted in this regard.)

One additional factor – experimental attitude – is particularly crucial when the subjects are aphasia victims, and it deserves special mention. For a number of reasons, varying from patient to patient, syndrome to syndrome, brain damage very frequently impinges on emotional stability; radical mood shifts are not uncommon. Severe depression, understandably, is often a consequent of aphasia, and Broca patients are the most susceptible – both in terms of frequency and of magnitude (Robinson and Benson [1981]). This is probably because they are the victims most aware of their communicative deficit, but there are also indications of chemical differences, of an increase in biogenic amines (Robinson and Benson [1981]). Overwhelming fatigue can also attend aphasia. Tasks that would have been trivial to the patient premorbidly can be completely debilitating. These variables potentially affect the way in which the subject views both experiment and experimenter; consequently, results can be badly skewed. There are two principal controls here. The task should be made as undemanding as possible, given the information sought. And the patients must be treated in a respectful, uncondescending manner, followed by a careful reporting of their performance during the task. Every effort has been made in these areas as well.

The crucial proviso here is that experimental control takes precedence over statistical control; indeed, statistics are treated with some suspicion in my analysis, and the experimental design was adopted to facilitate a brief series of *t*-tests.

1.1.3 Broca's Syndrome

The apparently glib designation above of Wernicke's aphasia as a foil to Broca's aphasia was not done lightly. The two syndromes involve very irregular looking symptom complexes, but in contraposition both begin to look far more regular. Isolated they appear chaotic; together they fall into recognizable patterns. Wernicke's aphasia is defined as a disorder signalled by fluent yet aimless production with intact functor and morphological distribution, by phonemic and semantic paraphasias, jargon, neologism, wordfinding difficulties, and by significantly impaired comprehension. This is not an appealing group of symptoms for a linguist. It indicates only that something profoundly wrong has happened to the victim's language capacity, but what that something might be is far from apparent: phonology, lexical access and semantics are all implicated. The same disarray is also evident in the symptom cluster commonly cited for Broca's aphasia: dysfluent yet pointed production, with a marked paucity of functors and inflectional morphemes, phonemic paraphasias, frequent misarticulations, word-finding difficulties, and relatively good comprehension. Syntax, morphology, phonology, and lexical access are all implicated.

However, as the somewhat biased phrasing indicates, if the two disorders are compared to one another both take on more definite form. In particular, the dimensions of production and perception are seen to be selectively impaired, in almost precisely opposite ways. Wernicke victims speak smoothly, with negligible content, and understand poorly; Broca victims speak haltingly, with content but negligible filler, and understand well. It is this dichotomy that generated Carl Wernicke's postulation of distinct aphasias, characterized by the interruption of sensory or motor capacities in language use, and it continues to fuel the division maintained by modern neurolinguists – though they tend to favour characterization in terms of semantics and syntax. In short, these syndromes are the primary focus for all theorists interested in promoting a central division in the aphasias. And of course there is anatomical support: Wernicke's aphasia is associated with damage to regions posterior to the Sylvian fissure; Broca's aphasia is associated with lesions immediately anterior to that fissure, and to the subcortical extentions of that region (Levine and Sweet [1982]). Clearly it is not requisite that injury to distinct areas results in distinct dysfunctions, nor that distinct dysfunctions be consequent to locally distinct lesions. Nevertheless, there is a certain formal elegance in site-symptom correlations, and elegance is always an important consideration in science.

On the other hand, it is obvious that a fair amount of overlap has been ignored for the sake of neat opposition, and the difference between the symptom complexes of the syndromes is far from binary. Phonemic paraphasias and word-finding difficulties are common to both disorders, and appear to pattern as a function of overall severity, not in terms of lesion site or of other symptoms. Such overlap, for theoretical if not clinical purposes, might best be viewed as the general result of introducing noise to the system – particularly as both slips of the tongue and temporary anomia are characteristics of normally functioning language users when the noise level goes up (due to fatigue, distraction, chemical agents, etc.). That is, not every symptom associated with a disorder need be considered a unique reflex of that specific disorder. Fever is a reaction to widely diverse bodily assaults, ranging from viruses to broken bones. It signals that something is awry, not that the right bimaleolar is fractured. In general, symptom complexes are meaningful in a way best captured by Wittgenstein's (1958) family resemblances: "a complicated network of similarities overlapping and criss-crossing; sometimes overall similarities, sometimes similarities of detail".[3] Again it is the overall pattern, not the individual details, that is of interest.

The overall pattern of Broca's aphasia just cited is the working definition of this thesis, and there will be occasion to invoke the Wernicke pattern just cited as well. One characteristic of Broca's syndrome in particular – agrammatism, the dysfluent, telegrammatic production symptom – is taken as criterial. There are two reasons for this: (1) agrammatism is unfailingly associated with aphasias consequent to Broca region insult; and (2) it has been the focus of much recent linguistic debate. Specifically, the claim that agrammatism is attended by a parallel comprehension deficit has become commonplace. Agrammatics are said to have the same difficulty understanding functors and inflections as they do producing them. These uniform productive and perceptive deficits are then, in turn, frequently taken in the literature as manifestations of a uniform competence deficit.

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[3] "Ein kompliziertes Netz von Ähnlichkeiten, die einander übergreifen und kreuzen. Ähnlichkeiten im Großen und Kleinen", *Philosophische Untersuchungen* I.66. Translation by G. E. M. Anscombe. Marshall (1982:405), in a very good paper on the nature of aphasic symptom clusters, also uses Wittgenstein in this context.

1.1.4 Knowledge and Implementation

This buzzword, *competence*, introduces another issue that requires some attention. Along with its perennial companion, *performance*, it is at the centre of a principal controversy in linguistics generally, and aphasic studies particularly. They are labels, respectively, for the theoretical cognitive state, knowing language, and for the class of peripheral mechanisms required to implement that knowledge.

A simple dichotomy is most probably a gross underestimation of the links between knowing and doing, but at the very least it is reasonable to assume that: (1) a person who speaks and understands a language has an internal representation of the principles and procedures necessary to talk and understand; (2) there are mechanisms which differentially access and utilize that knowledge; and (3) the former, in normal adults, is relatively steady-state, while the latter is subject to numerous variables (fatigue, distraction, chemical interference). Points (1) - (3) are occasionally contested, but they seem uncontroversially straightforward to me and are accepted hereafter without further debate. In any event, the available literature is extensive enough to license a graceful bowing out.[4]

Once the distinction is accepted, the issue is localized to whether a competence or performance disruption is responsible for a given aphasia syndrome, and what evidence bears on the matter. Consider first the rather crude analogy of a Tour de France athlete who loses the use of his legs through spinal damage. He would still have the knowledge of how to ride a bicycle, but his ability to implement that knowledge would be gone. For that matter, he would still have the motor programs for walking, but would be unable to walk. On the other hand, if he lost the ability to ride or walk through a blow to the head, it could no longer be legitimately claimed that his bicycle competence was gone or that his performance was intact. A neuronal relay to the middle ear might have been obstructed, for instance, incapacitating his ability to monitor balance. Access channels to the knowledge store might have been disrupted. In short, any number of disturbances aside from an obliteration of competence could account for the lost ability. Actual destruction of the knowledge, since it is only one link in a very long chain, is perhaps the least likely explanation; certainly it is the most difficult to confirm. The question becomes more dicey for a neurologically damaged cyclist who can walk but cannot ride, or ride, but not with his former proficiency. The conclusion that he has a selective deficit to his bicycle riding knowledge store is somewhat more easily justified here, but it is still only one alternative, and still very troublesome to demonstrate.

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[4] Cf. Watt's (1974) survey and discussion.

Language use is a far more complex activity than riding a bike, of course, and storage of the principles necessary to speak and comprehend probably occupy a good deal more cerebral space. Consider, then, another analogy. Consider the case of someone with a motor strip lesion which results in the lost control of one arm. It is functionally useless, rapidly atrophies, and remains clenched against one side. Yet, when she becomes tired, and yawns, her arm occasionally lifts, stretches out and flexes in a way she could not have willed. An exactly parallel situation occurs in language impairment. It is a clinical commonplace that aphasia victims who have much difficulty producing *dog* and *cat* can fluently and automatically turn the air blue with less polite monosyllables; in Hughlings Jackson's apt terminology, *propositional* language is disturbed, while *emotional* language is preserved (Jackson [1958:160, 161]). The damaged area subserves a facet of intellectual language production in a way that it does not subserve swearing. Yawning, a respiratory reflex, and cursing, an emotional reflex, are in both cases 'located' somewhere other than the damage site. Here is incontrovertible evidence that the motor knowledge for yawning, and for emotional outbursts, and all their performance mechanisms, remain neurologically intact. Clearly, the performance mechanisms shared by yawning and throwing a baseball, and those shared by speaking and swearing, are also intact. But there is still nothing obvious to be said about what is lost. Certainly there is a possibility that mental representations have been erased; yet it remains the least likely possibility, and the most difficult to verify. This introduces something of a truism in neurology: it is possible to demonstrate the presence of knowledge, but not its absence.

One more analogy, and the reader's patience, is required:

> Imagine a person who knows English and suffers cerebral damage that does not affect the language centers at all but prevents their use in speech, comprehension, or let us suppose, even in thought. Suppose that the effects of the injury recede and with no further experience or exposure the person recovers the original capacity to use the language. ... Suppose there is a second aphasic like the first, but because of some other and irrelevant problem (say, a circulatory disorder) he never recovers speech. Should we say in this case that the knowledge of English was lost? That would seem perverse. (Chomsky [1980:51-2]).

Indeed the claim would be perverse. No less perverse, for anyone schooled in the literature of aphasiology, is the proposed existence of two such abstractions. Chomsky goes on to suggest that "all sorts of evidence" might demonstrate retention of linguistic competence – for instance, "electrical activity or evidence from autopsy" (52) – and thereby reveals his lack of schooling in the area. But he does not have any empirical work to cite, using these methods or any others. And there is no mystery to the omission. No such cases have been documented.

The argument comes around, then, to why this section is so littered with analogies, of which Chomsky's is the *reductio ad absurdum*: there is no empirical support for competence models of language disorders. There are many that are not subject to disconfirmation by empirical means (indeed, most), but none that are uniquely supported by the data. Nor can there be until a much better understanding of neural circuitry develops. In the meantime, other hypotheses *are* open to testing, and factors like attention, short term memory, and temporal resolution can be fruitfully explored.

All this is by way of saying that competence is a valuable linguistic concept, and it is valuable for the reasons familiar from the arguments of its supporters: scientists need ideal, unattested constructs in their work. To use an example close to home, Fant's Source-Filter theory employs tube models with absolute rigidity and frictionless walls in order to understand the resonance properties of the vocal tract and predict the acoustic values of speech sounds. Grammatical models require the same distance from reality in order to explore rule-governed language behaviour and predict the grammaticality values of utterances. But the problems of aphasia are too immediate to be written off as irremediable knowledge deficits, interesting only by way of their implications for competence theories; that would be perverse. Since knowledge deficits cannot be demonstrated (even Chomsky's passage implies this), there is further perversion involved in their assumption, and insofar as the issue can be resolved, there is much aphasiological evidence of intact competence. Victims of Broca's syndrome, for instance, generally comprehend well and are very often painfully aware of grammatical deficiencies in their production. That is, they detect their productive deficiencies, and consequently can be said to have some knowledge of the correct output. But the crucial point here is simply that competence loss should be the last avenue explored. Ideal constructs are a luxury that aphasiology, and aphasia victims, cannot at present afford.

1.2 The Experiment

The experiment at the heart of this thesis, then, seeks to demonstrate the presence of knowledge and the impairment of implementation. The competence proposals for Broca's aphasia generally take this form: some intrinsic, definitional property of function words renders them unavailable to victims of the syndrome. The most common manifestation of this proposal is that the disorder is syntactic, and that Broca's region directly subserves a syntactic module. Presumably this bias is an outgrowth of the massive impact Transformational Grammar has had on linguistics, since TG analyzes sentences into *major lexical items* (nouns, verbs, adjectives, and adverbs) and *grammatical formatives* (everything else; see, e.g., Chomsky [1965:65]). That is, the theory considers all elements other than the (base form) four major categories to perform principally syntactic duties.

The evidence which is brought to bear on this functor deficit hypothesis comes primarily from experimental research into the comprehension difficulties of Broca victims. A particularly clear example of this evidence is Heilman and Scholes (1976), an experiment which managed to place an unusally heavy information load on *the* and found that Broca victims could not reliably detect that load. The argument from such data is that the function word in question is not consistently produced by victims of the syndrome, nor is it dependably perceived - therefore, it is unavailable to them. But, as above, reasoning along such lines requires a leap of faith. It is not possible to demonstrate the absence of knowledge.

Moreover, function words have properties other than syntactic ones. For instance, they generally have a much lower, more abstract, semantic value than content words, and there is some evidence that nonconcrete words are more problematic for aphasics than concrete words. Functors also receive sentential stress very rarely in normal production, while contentives are only rarely without stress in English, and there is a good deal of evidence that aphasics attend more closely to the stressed elements of a sentence. Either one of these factors, or a complex of both, might be responsible for the comprehension difficulties uncovered in work like that of Heilman and Scholes. My experiment tests the acoustic possibility, working with modified stress patterns and analyzing the affect those patterns have on comprehension.

1.2.1 Stress

However, modifying stress patterns raises some thorny theoretical issues and reintroduces the notion of competence. Prosody in general entails "a certain degree of vagueness" (Lehiste [1970:1]), and stress is the most mercuric of prosodic features. To start with, it has at least three manifestations: lexical stress, primary sentential stress, and specialty (contrastive or emphatic) stress. It also has three acoustic dimensions, each of which is a necessary property of any auditory communication. Every element of speech must have some value on a temporal metric, must have amplitude, and must occur at a characteristic frequency (even quiescent segments are defined in these terms, having a 0 on all scales). So stress is necessarily a relative phenomenon. Further, it is the clear product of a multilevel conspiracy. On the simple physiological level, there "is no single mechanism to which the production of stress can be attributed" (Lehiste [1970:106]). But there are also obvious lexical, syntactic, and pragmatic factors, in addition to such general notions as rhythm and tempo.

These problems of definition compound somewhat in an experimental paradigm, since even with the impressive current technology of speech modifiers and synthesizers, there is no effective way to "automatically modify the intonation profile of pre-recorded speech" (Francis F. Lee, personal communication). That is, generating stimulus material, for theoretical reasons, is no easy task. Most aphasic studies of stress take the specialty route, recording stress and control stimuli separately and highlighting one or more elements in the stress condition by speaker-regulated "extra prosodic emphasis" (Pashek and Brookshire [1982:379). This method has its drawbacks: increasing the acoustic values of one word in a sentence by speaker regulation simultaneously reduces the values of other words, particularly those in the immediate vicinity. When the stress targets are functors (and the subjects victims of aphasia), this effect could run substantial interference, by reducing the intelligibility of content words, and the comprehensibility of the sentence.

Fortunately, most of these problems are neutralized in the present study, since stimulus production is by digital means and stress is involved solely as an incidental notion. No claims are advanced here with respect to the linguistic nature of stress. Stress is an abstract concept, and probably best explained in competence terms: there are principles of assignment that must have cognitive representation and must have reference to language specific features of phonology. But these facts are irrelevant to the present concerns. This thesis is only interested that the acoustic properties of stress effect a saliency that facilitates comprehension, and that items of relatively short duration, relatively low amplitude, and relatively indistinct f_0 (i.e., nonstressed sentence elements) are potentially more difficult for psychoacoustic analyzers to process. So a precise definition of stress is of very little consequence. Nor are the diverse factors operating on stress in normal production of concern. It is not even of particular interest that *salience* is as slippery a term in linguistics as *stress*. The only relevant point is that two conditions are employed with different acoustic values for the same lexical items. The manner in which those conditions correlate with stress – though one is an unnatural hybrid between primary and emphatic stress – is not at issue. Stress patterns are necessarily altered but the study is unconcerned with those patterns, especially as they might implicate suprasegmental phonology.

Some very interesting, and wholly independent, evidence that such things as precise definitions and mechanisms of assignment in production may be irrelevant to the function of stress in comprehension comes from the study reported in Woods (1982). Woods is involved in a quite extensive artificial intelligence research program on computer implemented speech recognition. The model his team uses, dubbed HWIM (Hear What I Mean), proceeds by way of rather conventional spectrographic representation: incoming waveforms are plotted in terms of frequency, amplitude, and duration. An 'acoustic phonetic analyzer' then processes this information to form a 'segment lattice', which is examined by the control centre in concert with lexical, syntactic, and pragmatic components. The output, deriving 'utterance level hypotheses' about the representation along a path laid out by nondeterministic strategies, is effectively translated into orthography. Rather than adopting a sequential policy, HWIM has proven very successful by scanning for 'anchors', for particularly reliable pattern clumps on the spectrogram, assigning structure to surrounding material as a function of the anchor's properties, and generating sentential hypotheses through comparison of one anchor to another.

There are two findings of this research which are particularly relevant to the Broca's syndrome comprehension problems:

> [1] We learned that small function words are highly unreliable anchors. For example, the sound in *a* is usually the same as that of the vowel in *the*, and it also occurs in many multisyllabic words.
>
> ...
>
> [2] We found that sequential left-to-right scanning has problems. It is often necessary to provide the ability to recover from a garbled word. The first word of the sentence is often garbled due to high subglottal pressure. But right-to-left scanning is not a viable alternative, as the last word of a sentence can be garbled due to low subglottal pressure that, for example, lengthens phonemes. We thus often find it expedient to work out from those "islands of reliability" provided by the stressed syllables away from the

ends of the utterance. (98)

The implications here of HWIM for cognitive speech analysis are obvious: stress provides focus on the parts of the signal which are more crucial to the message, and unstressed material (like many functors) is secondary. A less efficient - damaged - analyzer might ignore unstressed material almost entirely.

Analogies from machines to minds are often very tenuous (see, e.g., Pylyshyn [1980], Calvin [1983b:53-62]). But in this case, the move is from an acoustic analyzer to an acoustic analyzer, not from computation to cognition, and consequently is far more concrete. The only important assumption required for the analogy to go through is that humans process speech signals in terms of frequency, amplitude and duration, an assumption made throughout this thesis and motivated by the entire discipline of phonetics. Further, the HWIM model simply affirms the widespread, common-sense notion of the function performed by stress: it "is used to indicate the most important word or words in a sentence or phrase" (Ainsworth [1976:90]); that is, stress is a tool of salience.

1.2.2 Summary

The primary objective of this thesis is to discover if functors can be made more salient acoustically for victims of Broca's aphasia, and to that end the stimulus modifications follow the lead of the primary agent of salience in language, stress. The hypothesis tested is that the comprehension difficulties Broca patients have with function words are, at least in part, a product of perception, signal analysis, or storage, and not strictly (if at all) of a competence disruption. If this hypothesis is correct, the performance of Broca victims on a task keyed to the comprehension of a functor should improve significantly. In turn, this would demonstrate that acoustic factors – performance variables – are to some extent responsible for the comprehension difficulties that follow from neural insult to Broca's area.

The paradigm for this test is that of Heilman and Scholes (1976), which is outlined in some detail in the following chapter, and very explicitly in Chapter 3. There are two principal reasons for this choice. The first should be self-evident: since it supplies some of the most compelling data in support of the functor deficit hypothesis, it requires, at absolute minimum, one replication to see if the results are stable. The second reason should become apparent when the paradigm is described in some depth: it is a very elegant design, and licenses a very direct test of the perceptual hypothesis. The subjects were played two versions of the Heilman and Scholes stimulus sentences, one which had normal stress assignment, and a version identical to that except for the substitution of a longer, louder *the* in the key location.

It might be argued that this replication is too safe: failure to reproduce Heilman and Scholes' results would compromise their competence hypothesis; reproducing them without a saliency effect would substantiate important work conducted a decade ago with a small subject pool, and it would support their hypothesis; and reproducing those results with a saliency effect would help circumscribe the Broca comprehension deficit, and compromise the competence hypothesis. But this surety of meaningful results, irrespective of the outcome, is a virtue of the design, not a flaw, and there remains a hierarchy of usefulness in the three possible outcomes.

The third result, replication with a saliency effect, is the most useful. If function words are difficult for Broca patients to understand because of their weak acoustic values, then speech therapy is clearly of value, and the established clinical practice of slower, louder, more careful enunciation makes sense. If, on the other hand, those difficulties follow from a deep-rooted competence disruption, therapy makes less sense, and clinical enunciation makes none. Fortunately, it was this result the experiment produced. Heilman and Scholes' findings held for the normal condition, but the patients were significantly more reliable when the key functors were acoustically boosted.

2. Literature Survey

This chapter observes one major division in its treatment of the relevant literature: experimental vs. theoretical. Adopting this distinction is not meant to imply that these two approaches are mutually exclusive, or even incompatible. Theory that does not rest on empirical research is no more scientifically compelling than, say, the Book of Genesis. Indeed, creationists are correct to point out that Genesis *is* theory; but it is not science. Similarly, experimentation is not of particular interest if it does not speak to theory, if it haphazardly generates information with no frame of reference.

But the two approaches are nonetheless distinct, and the contrast is sharper in aphasiology than in most fields of inquiry. In particular, theoretical linguistics which broaches aphasia is wont not to draw very heavily upon experimentation, or to do so only with suspicious selectivity. As that last sentence, and much foregoing discussion should indicate, the bent of this thesis is decidedly empirical. The experimental survey below is comprehensive: all the principal work, and some incidental work, on perception, discrimination, and comprehension of temporally and stress manipulated stimuli is addressed in detail, as is the work on the function word deficit in Broca's aphasia. The theoretical survey is more exclusive. It considers only two accounts of Broca's syndrome: Edgar B. Zurif's proposal of a syntactic deficiency, and

Mary-Louise Kean's proposal of phonological impairment. The first is chosen for its prominence, and for its attention to function word experimentation; the second is chosen because the experiment that motivated this thesis might, superficially, be seen to have some verificational power with respect to it. This is not the case.

2.1 Experimental

As with the overall organization of this chapter, there are also two principal divisions in this subsection – since there are two classes of experimental evidence which bear on the question at hand. First, there is a set of studies based on the physical manipulation of auditory signals. This group includes various methods of temporal alteration – such as slow speech, interpolated silences, and periodic repetition of brief signal samples – as well as direct controls of stress. The stress experiments, of course, are of the most immediate relevance, but stress is a much more difficult property to tame effectively than is duration. Consequently, there have been proportionately fewer studies. However, one of the features of stress – according to Fry's (1955) early experimental work, the single most important feature – is increased syllable duration; so the rather wide body of work done in this area, running the gamut from sinusoidals to poetry, is also of considerable interest.

There is some variance in the results, as is inevitable in language pathology experiments, but in general the physical manipulation work argues quite persuasively towards one conclusion, all the more compelling for the amount of data amassed: aphasia entails a breakdown of acoustic signal processing. Language comprehension is therefore facilitated for aphasia patients by exaggerating stress and by temporal expansion. Unfortunately for the purposes of this thesis, very little of the temporal alteration data are analyzed in terms of the symptom clusters outlined in the first chapter. The subtyping metric, if any, is usually a measure of total severity, graded with such tools as the Minnesota Test, the Sklar scale, the Porch Index of Communicative Ability (PICA), or the Token Test (TT). Consequently, it is rarely possible to discover whether or not clusters like Broca's or Wernicke's aphasia interact significantly in the results.

On the other hand, the second set of studies, investigating functor deficits, is virtually predicated on these clusters. Most of the experimenters have theoretical axes to grind – usually some form of the claim that Broca's aphasia is (or is not) a syntactic deficit – and the experiments serve as the grinding wheels. Still, motivations aside, lesion site and performance characteristics are very useful information to have for an aphasic subject pool; all the more so when they correlate. More importantly, the results of this work are also quite convincing. That is, victims of neural insult matching the symptom complex labelled *Broca's aphasia* in the first chapter evidence a comprehension deficit peculiarly implicating functors.

2.1.1 Perception and Discrimination of Temporal Domain Signal Characteristics

The first group of experiments concerning aphasic perception of temporally altered acoustic signals begins with the work of Robert Efron.

Efron (1963)

Efron's principal interest was in the hemispheric localization of the ability to sequence discrete events, but some of his efforts to this end involved the testing of aphasia patients, and his results show the key role played by duration in their perception. In particular, Efron (1963) demonstrates that increasing the interval between acoustically distinct signals facilitates aphasics' ability to correctly determine the sequence. That is, when subjects were exposed to two randomly ordered frequency pulses – one "a high-pitched 'beep'", the other "a low-pitched, somewhat rasping 'bop'" (405) – aphasics required a longer intervening period of silence than controls to accurately identify which came first. Neurologically damaged nonaphasics performed the task efficiently (100% correct identification) at a mean interval of 130 msec; the aphasics needed an average interval of 460.23 msec. This difference looks substantial, but two points must be noted: (1) homogeneity of variance is unsupported (Bartlett's test), and consequently standard tests of statistical significance are unavailable; and (2) one of the aphasics fell within the range of the control group.

However, much of the variance within the aphasic group is acounted for by the presence of three distinct syndromes. Once they are separated out, the homogeneity hypothesis is supported (Cochran's test), and the data are more amenable to statistical inquiry. The three syndromes, by Efron's taxonomy, are: *expressive aphasia* (6), *receptive aphasia* (4), and *unclassified* (1). The first two labels correspond to *Broca's aphasia* and *Wernicke's aphasia* respectively, as those terms are defined in the preceding chapter, and the third matches the Boston definition of *Global aphasia* (cf. Efron's clinical apraisals on p.410, and his lesion localization discussion on p.415). Since there was only one global aphasic, Efron does not include his/her data in subgroup analysis, but the remaining three groups fall into an interesting pattern: Broca's aphasics require a significantly longer duration than Wernicke's aphasics, who in turn require significantly longer than the controls. The results are summarized in Table 2.1. Two tailed t-tests show all the differences to be significant at the $\alpha=0.01$ level, and an expressive vs. control test is significant at $\alpha=0.001$. Further, as the distribution of means should make clear, the aphasic outlier mentioned above is a member of the receptive group. Broca's area appears to involve some dependency on temporal signal resolution.

Table 2.1

RESULTS OF EFRON (1963)

	75%	100%
Control subjects	75	130
Receptive aphasics	140	235
Expressive aphasics	400	575

Column 1 is the mean threshold for 75% correct responses, in msec. Column 2 is the 100% threshold. (Taken from pp. 409,410.)

Efron takes these findings to suggest that aphasia is the reflex of a general impairment of the ability to temporally organize the discrete elements of an input signal, explicitly rejecting a competence analysis:

> If this view is correct, we should not look upon the aphasias as unique disorders of *language* but rather as an inevitable consequence of a primary defect in temporal analysis. ... Aphasia, thus considered, would be an epiphenomenon. (418; Efron's italics)

A sequencing impairment is by no means necessary – a failure of discrimination, for instance, is just as likely a candidate – but the results do support a claim that extended duration plays a more substantial part in signal analysis for aphasics than nonaphasics, and this role may well be independent of linguistic competence. They also suggest that damage to Broca's area implicates a greater dependency on signal duration than other types of brain damage.

Efron's basic paradigm has since been replicated twice. In each case a different limitation of the original design was addressed.

Edwards and Auger (1965)

The first of these replications, Edwards and Auger (1965), attempted to locate deficits in auditory perception which might be coincidental with, and consequently mitigate, Efron's results. They administered his test in concert with a battery of other auditory measures: "the modified Pitch, Tonal Memory, Loudness, Rhythm and Time subtests of the Seashore Form A (1939)" (207). The subject pool was also expanded – including 20 aphasics, 20 neurologically damaged and 20 neurologically sound nonaphasics – though unfortunately no aphasic subtype analysis was done on the data. Of these six tests, only the Efron sequencing task produced significantly different means for any two of the groups: at the 75% threshold level, the normals required an interstimulus interval of 67.49 msec; the neurologically damaged nonaphasics required only 50.79 msec, and the aphasics needed 141 msec. It is not clear from the discussion (since no standard deviations are supplied) whether these means are all significantly different, one from the other, or whether it is just the aphasics who differ from the two controls. But an analysis of variance yielded an F-ratio with $p<0.001$, and it is the aphasic results that are most prominent. The aphasic subjects required an interval substantially longer than that of the other two groups combined. Efron's findings hold, independent of pitch, tonal memory, loudness, rhythm, or time deficits.

Further, although no subtype analysis was performed on the aphasic data, there is some reason to believe that subjects meeting the symptoms of Broca's aphasia did less well than Edwards and Auger's counterparts on the sequencing task. A short digression on their evaluation tool, the Minnesota Test, is required; whatever its clinical merits, it has problematic areas. In particular, it is not designed to uncover the kind of symptom clusters of concern to most aphasia theorists. Instead, the test looks for functional language impairments and seeks a prognosis of readaption to society. Since comprehension is far more crucial to social integration than fluent output, the test's seven point *simple* to *irreversible* scale tends to run from productive to perceptive deficits. *Mild Aphasia with Persisting Dysfluency*, number 3 on the scale, meets most of the criteria set out in the first chapter for Broca's syndrome; *Aphasia with Intermittent Auditory Imperception*, number 6, meets most of the criteria set out for Wernicke's syndrome. All this is by way of pointing out that thresholds for the sequencing task had a relatively strong negative correlation (-0.56) with total severity of disorder, as measured by the Minnesota Test. That is, mild (Broca-like) aphasics required more time for the task than more impaired (Wernicke-like) aphasics.[5]

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[5] On the other hand, Edwards and Auger report that an "intense investigation of the unexpected sign reversal was conducted by a qualitative analysis of the neurological status of the aphasics. No orderly relationships could be

2.1.2 Perception and Discrimination of Temporal Domain Speech Signal Characteristics

Ebbin and Edwards (1967)

Ebbin and Edwards' (1967) replication attempted to discover if Efron's (1963) generalization from pure tones to speech sounds was justified. Consequently, they substituted syllables for frequency pulses and a discrimination task for sequencing. Again the subject pool was larger (24 aphasics, 24 neurologically damaged controls), and again no subtype analysis was done on the data.[6] The stimuli employed were CV structures consisting of either a stop [p, t, d, k] or a fricative [s] and "the vowel sound [ɔ] as in father" (121). Twenty-five combinations were generated under two conditions. In one condition (0 msec), there was virtually no interval: "the second syllable following the conclusion of the first syllable as closely as the physical limitations of splicing would permit" (122). The other condition included a 200 msec interval between stimuli. The number of errors per condition was measured, and the results fall into line with the earlier studies. Nonaphasics had some difficulty with the task, but their problems were independent of condition; in fact, they did marginally

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[5](cont'd) detected" (208). The wording is muddy, but this passage may mean (depending on "qualitative analysis" and "neurological status") that lesion site and performance on the sequencing task showed no correlation.

[6] The Minnesota Test was once more the evaluation metric, and Ebbin and Edwards interpret its results as diagnosing their average aphasic subject as suffering "mild impairment" (121). Following the above logic, this indicates that most of their aphasics were Broca patients.

better on the 0 msec condition (20.7 vs. 21 mean errors). The aphasics had more difficulty overall, and there was a significant treatment effect. When the syllables were unseparated the aphasics averaged 34.15 errors, but with the 200 msec interval the mean dropped to 29.35. This is precisely the trend predicted by the Efron results – increasing the duration also increases accuracy of judgement – and the difference proved significant at $\alpha=0.001$.

In short, expanding the duration of a signal with interpolated silence makes a considerable difference in the reliability of aphasic perception, and at least one experiment (Efron's) shows that it makes more difference for Broca's aphasics.

Carpenter and Rutherford (1973)

A similar, though far better controlled, consonant discrimination experiment, by Carpenter and Rutherford (1973), also tested aphasics against neurally intact and neurally insulted nonaphasics. The aphasic population was subtyped but of very uneven distribution (nine Broca's, two Wernicke's, two amnesic, and one unclassified), so group comparisons could not be profitably made. However, the high proportion of Broca patients among the subjects makes the study of particular interest.

The tests were also subtyped: three were classified as *spectral*, and concerned the frequency characteristics of the signal; three were classed *temporal*, and concerned cue duration (the lengths of stopgap [closure], frication, and vowel nuclei). The results, presented in Table 2.2, are very much in line with the earlier work.

Table 2.2

RESULTS OF CARPENTER AND RUTHERFORD (1973)

Subtests	Aphasic	Normal	Nonaphasic Brain-Damaged
Spectral			
tail/pail	87.7	90	100
fig/fib	100	100	100
shake/shape	40	100	100
Temporal			
stable/staple	40	100	90
base/bait	20	80	80
hid/hit	13.3	100	70

Percent correct discrimination. (Taken from p. 538.)

Both control groups did well on all the measures of spectral discrimination, and the aphasics did well on all but the *shake/shape* subtest. The control groups also did well on the discrimination of temporal cues, but the aphasics did very poorly here. Carpenter and Rutherford demonstrate statistical significance in two key dimensions. First, performance of the aphasics on the temporal discrimination tasks was significantly worse than either of the control groups ($p<0.005$). And second, aphasic discrimination of temporal differences in the signal was significantly worse than their discrimination of spectral cues ($p>0.001$). Further, there may well have been temporal contamination of the one spectral test aphasics scored poorly on. The principal frequency cue for the distinction of /ep/ and /ek/ is the second formant transition, and the authors admit (538) that their f_2 transitions on this task were very brief (<50 msec).

Sasanuma *et al*. (1973)

A convenient bridge between the temporal experiments using pure tone stimuli and speech sounds is provided by the work of two independent research teams. The first team, Sasanuma, Tatsumi, Kiritani, and Fujisaki (1973), studied the perception of both signal types by the same (Japanese) aphasic population. Unfortunately, they used a very small control group (n=4; against 29 aphasics), so the between-group findings, though they demonstrate the expected inferiority of aphasic discrimination, are statistically suspect and must be rejected. But a very useful result does surface: a high correlation for the aphasia within-group, between-task findings.

The subjects were required to discriminate (1) 200Hz pure tones of varying duration (100 - 400 msec); (two) a two vowel sequence which was manipulated for duration of the initial vowel (120 - 240 msec), and which corresponded to two Japanese words (oi/ooi); and (3) a VCV sequence which was manipulated for duration of closure (110 - 230 msec), and which also corresponded to two Japanese words (ita/itta). The linear regression figures – showing increased discrimination as a function of increased duration – are virtually identical for all three tasks.

Tallal and Newcombe (1978)

The second 'bridge' study, by Tallal and Newcombe (1978), is considerably more detailed, and statistically more opaque, but produces the same practical results. They tested 10 aphasic patients on the perception of four synthesized acoustic pairs. One pair was "nonverbal": complex tones of identical harmonics but distinctive in fundamental frequency. The other three were "verbal": (1) steady state vowels /ɛ/ and /æ/; (2) CVs /ba/ and /da/, with 40 msec formant transitions; and (3) the same CVs with 80 msec formant transitions. All stimuli were 250 msec long, and the interstimulus interval varied from 8 to 428 msec.

The aphasic patients proved significantly worse in all tasks ($p<0.001$) than both the neurologically damaged and neurologically sound control groups (between whom there was no statistically relevant difference). As in the earlier studies, there was also a significant within-group difference for the aphasic population in the perception of sequences with a short interstimulus interval versus a long interval – performance being much better with increased duration ($p<0.001$). The effect of formant transition duration was less widespread, but some of the aphasics demonstrated improved performance with the 80 msec cues.

Sasanuma *et al*. (1973) provide a succinct statement of the importance of the temporal discrimination experiments with non-speech stimuli: they suggest "the presence of a certain kind of 'noise' or defect in the auditory system of aphasic patients even at a stage preceding speech comprehension" (72). That is, returning to the competence / performance distinction, there is clear evidence of a performance deficit and no evidence at all for a defect of competence (however that might be found) – a point stressed by Efron (1963). Further, there is a close correspondence between perceptual difficulties with dissociated frequency pulses and with phonologically valid strings.

2.1.3 Comprehension of Temporally Altered Speech

2.1.3.1 General Tests

An additional factor in the Sasanuma *et al*. (1973) experiments, though not controlled for, is the presence of signification. Their phonological discrimination tasks employed words, meaningful elements of the subjects' language. Since only discrimination was tested, there is no way of knowing whether the subjects perceived these stimuli as lexical gestalts or just as distinct phoneme (or perhaps, sound) strings, but several other experiments suggest that there are semantic implications to the impaired temporal processing abilities of aphasics. Tallal and Newcombe (1978) supply some evidence of these implications. While they had no semantic elements in their study, they did use a comprehension test for screening purposes, and a *post hoc* analysis revealed a very high (Spearman's) correlation between their nonverbal task and test scores (r=0.83). As they point out, results such as this "strongly suggest that impairment in responding to rapidly changing acoustic information is, in most cases, concommitant with receptive language impairment" (22). Sequencing and discrimination, of pure tones and of complex speech sounds, improve with physical expansion of the signal. The same proves true of comprehension.

Sheehan *et al.* (1973)

Sheehan, Aseltine, and Edwards (1973) applied Efron's (1963) technique of interpolated silences to meaningful linguistic strings. Their experiment is problematic, and in isolation provides little evidence that temporal expansion benefits aphasic comprehension. Specifically, neither the task nor its analysis is well defined. Still, the results are suggestive, particularly when viewed in concert with the other expanded signal work reviewed in this section.

Sheehan *et al.* made up tapes of "stories, poems, narratives, and so on" (652) under three conditions. The interpolated silences condition placed a noiseless gap of 150 msec between all "individual articulated phonemes" (652). The "accumulated time" condition left the words intact but separated them by stretches of silence, calculated as the sum of the potential silences of a given word (so that, e.g., a word with four internal silences under the first condition was separated by a 600, or 4 x 150, msec gap from the following word). The control tape contained the stimuli with "the experimenter normally enunciating every word" (652). Curiously, these tapes were then dubbed onto a videotape of the experimenter lip-synching the audio track. All subjects (n=30) heard all three conditions, and answered simple yes/no questions on the passage's content, recorded under the same conditions.

Subjects were subtyped solely by age, and the only interesting result was that patients under 50 showed significantly improved comprehension for the interpolated silences condition. The point cannot be pushed very far, but a number of studies have found that Broca patients tend to be younger than Wernicke patients (Brown and Jaffe [1975], Brown [1977, 1979, 1982], Obler *et al.* [1978]). The age of 50 is also important to these studies. For instance, Brown (1982:450f) reports on a study of 389 aphasics where the average "age of motor aphasics was 45.3, and fluent aphasics 56.5". However, age-performance may well be a correlation without causal connection here; the patients under 50 also evidenced substantially milder deficits (measured on the Sklar Aphasia Scale).

As above, the experiment was less than ideal, but this partially contributes to the significance of the findings: the test group was under quite severe handicaps in the interpolated silences condition, and would be expected not to understand it at all, yet the older subjects did no worse on it, and it was the best condition for the under 50 subjects. First, it is not at all clear how the 'individual articulated phonemes' were distinguished by Sheehan *et al.*, and the problems of mapping acoustic waveforms to discrete phonological entities is one of thorniest issues in phonetics.[7] Even more confounding is the authors' report that the interpolated silence condition for "normals ... is often incomprehensible until they learn recognition" (656); it is unnatural, and does not correspond to any familiar temporal alterations, like a slowed rate of speech or deliberate articulation. This makes the performance of both groups interesting: the older group's comprehension did not degenerate under the interpolated silences condition, as might be expected, and the younger group's comprehension improved. The younger group understood the interpolated silences condition better than normal enunciation, and better than the accumulated time condition, a more natural expansion. The difference is significant at $\alpha=0.02$. Further, both groups showed improvement upon second exposure. While the older group's improvement was marginal, the younger group's performance was significantly better: 55.6% correct responses on day 1, and 71.9% correct the following day ($\alpha=0.001$).

Gardner *et al.* (1975)

An ambitious foray into aphasic comprehension difficulties was conducted by Gardner, Albert, and Weintraub (1975). They attempted to measure a number of different factors in perceptive deficits, one being of particular interest here. Among their battery was a group of "neutrally worded" sentences containing "target nouns"; e.g., "You see a cat that is nice" (157). Presumably these were spoken at a normal rate with normal intonation, since nothing is mentioned to the contrary. The sentences were also presented at a rate of 60 words per minute. The subjects (46) were required to match the stimuli to a picture, from an array of seven, depicting the target noun. The slower rate of presentation yielded a highly significant improvement in performance on the matching task ($p<0.001$). The study also revealed a significant effect for site of lesion (no α-levels given), with anterior patients showing better comprehension. But, unfortunately, no specific information is given as to improvement ratios. That is, it is not possible to know if either the anterior or the posterior aphasics benefited more from the reduced rate of presentation.

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[7] For a recent review of the literature on this topic, cf. Blumstein and Stevens (1980). Notice that this lack of one-to-one mapping does not interfere with the Ebbin and Edwards (1965) experiment, since only discrimination and not comprehension was at issue.

Weidner and Lasky (1976), and Lasky *et al.* (1976)

Weidner and Lasky (1976) presented a group of 20 subjects with stimuli borrowed from the Minnesota Test, controlling for rate of speech. Four tasks were required: subtests IV, V, VI, and IX of the Auditory Disturbances section of the test, which comprise requests for simple actions, yes/no questions, and repetitions; factors of length and syntactic complexity are built into the stimulus sentences. All tests were carried out under two conditions:

(1) a 110 word per minute rate of presentation, and (2) a 150 wpm rate. The results pattern as the Minnesota Test predicts. Increasing length and complexity decreases the reliability of the patients' performance. But there was also a significant difference for rate of presentation, with by far the best performance under the 110 wpm condition; significance was beyond the 0.05 level.

Lasky, Weidner, and Johnson (1976) ran a slightly more detailed study of much the same order. In the Minnesota Test, syntactic complexity is not well-controlled – embeddings, conjunction, and adjunction are varied indiscriminately – but in this experiment only two dimensions were varied. The stimulus sentences differed only in voice and negativity; length also varied, since passives are longer than actives, negatives longer than affirmatives. There was also more control over presentation rate. Four temporal conditions were used: (1) a 150 wpm rate, with an interphrase pausetime of 1 sec (where phrase boundaries are defined as corresponding to the slash marks in sentences 1-3 below); (2) 150 wpm with no pauses; (3) 120 wpm with a 1 sec pause between phrases; and (4) 120 wpm with no pauses.

1. The grandfather / is kissing / the girl.
2. The player / is not hitting / the ball.
3. The dog / is chased / by the cat. (393)

The patients (15) again did uniformly better at the slower rate of presentation, and pause time also facilitated comprehension (though lack of control precludes saying anything about pause placement), so that the best condition was (3). Both effects – pause time and word rate – were significant beyond $\alpha=0.05$, as was syntactic complexity.

For both experiments aphasic subtyping was carried out by means of the PICA, so the only criterion was overall disability. In the first study subject results were analyzed in two groups, one consisting of all those subjects with a score above the 50th percentile, and the other group made up of those below. Not surprisingly, the group with the highest PICA scores also had the highest performance levels. Improvement as a function of decreased rate was likewise greater for this group than the under 50th% group. In the second experiment, the Porch Index appears to have been more of a screening tool, perhaps to boost overall results, since only those scoring above the 50th percentile were included, and no subanalysis was conducted.[1]

Goldfarb and Halpern (1981)

Goldfarb and Halpern (1981) conducted a conventional word association test with aphasic and normal subjects, presenting the stimuli under two temporal conditions: a 'normal' rate of 10 phonemes per sec, and a reduced rate of five phonemes per sec. Only the aphasic subjects (32; mild to moderate impairment) provided interesting variance in their response patterns. Under the normal rate, they produced a roughly even number of paradigmatic (same lexical category) and syntagmatic (related but different lexical category; e.g., an adjective for a noun stimuli) responses, with the edge going to the latter. But with increased signal duration, the difference became significant ($p<0.05$) and the majority of responses were paradigmatic (repetitious, anomalous, and unclassifiable responses remained relatively constant, though the number of unclassified responses dropped somewhat in the slower condition).

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[1] No figures are given for the two groups of Weidner and Lasky (1976), but if the same pool was used as for Lasky *et al*. (1976) then the *n*s would have been substantially unequal (15 vs. 5), making homogeneity of variance more difficult to support, and calling into question the significance of their *F*-statistic.

There is some doubt as to whether word association tests require comprehension (cf. Goldstein [1948]), but at the very least they appear to implicate a recognition of lexical category, and this ability improves as a function of temporal expansion for aphasics. More to the point, under the reduced presentation rate the aphasic responses move in the direction of normal response patterns; Goldfarb and Halpern's controls supplied twice as many paradigmatic as syntagmatic responses in both conditions.

Pashek and Brookshire (1982)

One of the few advantages Sheehan *et al*. has over the other experiments reviewed in this section is the inclusion of context, and their results are specifically validated by a better-controlled, recent run of a similar paradigm, Pashek and Brookshire (1982). Just as dissociated pure tones are further removed from natural language than are speech sounds, and speech sounds are further removed than sentences, sentences are less like natural language than are paragraphs. The argument here is not that people go around every day speaking in paragraphs. In fact, outside academia, we are more liable to speak in constituents shorter than sentences, not longer. But language is always subsumed by context. "The grandfather is kissing the girl" is a perfectly well-formed, comprehensible English sentence, but it is not likely to be heard unless that action is transpiring, and probably not then. The advantage of "stories, poems, narratives, and so on" is that they bring along their own context, a network of related propositions, so any given proposition is reinforced a number of ways, and is more likely to register with the hearer than if it were presented in isolation. There is some experimental work which establishes just this point for aphasics (cf., Wilcox, Davis, and Leonard [1978]).

Pashek and Brookshire (1982) used a much better defined stimulus set than Sheehan *et al.*, short paragraphs of 93 to 96 words that were gauged on the Dale-Chall Readability Formula to be at fifth and sixth grade reading levels. They also expanded these stimuli in a less artificial manner, having an experienced reader record them at rates of 120 and 150 wpm. The tapes were then tested for an overall accuracy of ±2 secs., and rerecorded as necessary. Comprehension was evaluated by eight pairs of yes/no questions, randomized, with each member testing the same proposition in a different format (truth value and/or wording; e.g., "Were Samuelson and Harbo fishermen by trade?" and "Were Samuelson and Harbo two famous doctors?"). There was no significant variance found in their (neurologically sound) nonaphasic data. But both groups of aphasics (20 overall; subtyped as high and low comprehension on the Token Test) performed markedly better on the slow rate of presentation than the normal rate ($p<0.01$). That is, the results are the same as those of Sheehan *et al.* – slowing the presentation rate of context-supported messages enhances aphasic comprehension – but it is a good deal easier to have confidence in them.

2.1.3.2 Token Tests

However, context is a double-edged blade in language studies. Its inclusion more closely approximates the way language is used daily; but language is rarely, if ever, employed independent of other cognitive abilities. Consequently, the inclusion of contextual information in a psycholinguistic study substantially restricts the certainty with which that study can be said to tap linguistic knowledge.

The Token Test of De Renzi and Vignolo (1962), used as a subtyping tool in the experiment just discussed, is a very elegant test for mild perceptual deficits, of the sort commonly associated with Broca's aphasia, and it is designed precisely to avoid these complications of context. It requires the patients only to manipulate on request a few simple geometric shapes of different sizes and colours. But the requests move from direct to quite subtle, necessitating comprehension of conditional clauses, word order, attribution, and a wide range of function words. The following examples should illustrate the ingenious simplicity of the test:

4. Pick up a circle.
5. Put the red circle on the green rectangle.
6. Pick up the rectangles, except the yellow one.
7. Instead of the white rectangle, take the yellow circle.

The advantage of geometric shapes is that they minimize the "involv[ment of] other intellectual functions than language" (665). They reduce the number of pragmatic constraints on the situation and place more of the information load on the linguistic string. For instance, if the task were to place a button in a cup, no matter how intricately worded the request, subjects would have a high probability of responding 'correctly' just by recognizing the nouns and surmising that some simple manipulation was required. A button and a cup limit the possible number of acceptable manipulations more than a circle and a rectangle.

De Renzi and Vignolo (1962:77-8) demonstrate the effectiveness of their test by using it to uncover the comprehension problems of "13 'pure' motor aphasics and six sensory aphasics at an advanced stage of recovery, none of whom had ever "shown any difficulty in understanding a normal conversation". Since everyday discourse is far more complex than a simple exchange of linguistically encoded information, the subjects could function adequately by drawing on their pragmatic resources. But they lacked full control of their language, and the Token Test located the areas of impairment.

Shortly after De Renzi and Vignolo's (1962) paper, Orgrass and Poeck (1962) published an independent evaluation of the Token Test, watching for correlations of score with age, sex, education level, or "behavioral defects in non-aphasic brain damaged patients" (242). None of these factors was found to significantly interact with the test, which demonstrates that it centres very well on language-specific deficits. A number of subsequent assessments have also been carried out (e.g., Boller and Vignolo [1966], Boller [1968], Swisher and Sarno [1969], and Poeck *et al.* [1972]), and all have returned the same favourable verdict. In brief, the test is short, well-controlled, effective, and investigates rather subtle deficits. This combination of features has made it a very attractive tool for experimental aphasiologists, and it has been employed in a number of paradigms. Of particular interest here are those experiments which control temporal aspects of the requests.

Parkhurst (1970)

The first temporal comprehension experiment was that of Parkhurst (1970), reported to the Annual Convention of the American Speech and Hearing Association, but unfortunately not published. Though several subsequent experimenters cite Parkhurst's work (e.g., Liles and Brookshire [1975:222], Blanchard and Prescott [1980:116], Pashek and Brookshire [1982:377-8]), conditions, analyses, techniques, and subgroup performance differences are not given in any detail.

Principally, Parkhurst found that aphasic comprehension of the test's commands is significantly poorer than that of nonaphasics when the signal is electromechanically compressed by 35%. Within the aphasic population, significant differences occurred only for the more difficult test sections, which compression rendered very troublesome. Some of the aphasic subjects also showed improvement when the signal was expanded by 35%, but again only for the more difficult items.

Liles and Brookshire (1975)

Trying a different tack than Parkhurst (1970), Liles and Brookshire (1975) ran a modified Token Test controlling temporally with the interpolation of five sec silent gaps. Normal token requests were matched against ones including a single gap in various locations, and the results proved quite interesting. First, a significant effect showed up between the expanded requests and their shorter, normal counterparts, just as earlier experiments predict. But location also played a role in the aphasia patients' comprehension. The subjects (20; all below the 90th% on the PICA) had a much easier time of it when the silences followed content words (adjectives or nouns) early in the sentence ($p<0.05$). Yet if the gaps followed functors (prepositions or disjunctions) anywhere in the request, or content words late in the sentence, interpolated silences improved comprehension only marginally (significant at the 0.25 level). This indicates that (1) increased processing time facilitates comprehension of semantic but not functional information, and (2) facilitation only occurs when the memory buffer is relatively unencumbered. The first indication is important here because it is functional information that gives Broca's aphasics the most trouble, though there is a good chance that interference occurred (functors are phrase initial and tend to cliticize, so there are very few natural gaps following function words in normal speech). The second is important because it implies a particular defect influencing aphasia, namely impaired short term memory; and both results show that simple overall signal duration has significant limits in its aid to aphasic comprehension. That is, there are other factors at work in signal expansion that improve aphasia victims' speech perception than increased length of utterance.

Marckworth (1976)

More in line with Parkhurst (1970), both in technique and results, is Marckworth (1976).[5] She administered the test to 12 subjects with "mild to moderate receptive aphasia" (82); most were also hampered expressively and only one met the clinical description of "fluent", so the experiment is of particular interest here. The stimuli were presented under two recorded conditions: "a normal speech rate" (85), and an expanded version of the same signal (at a ratio of 1.96 to 1, almost six times Parkhurst's [1970] rate). Extended duration was accomplished by means of a signal expander, which periodically repeats brief samples of the signal. This results in a product that is more 'natural' than either a technique of interpolated silences or slowed dictation.

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[5] I should also note here that earlier sections of this survey are in debt to Marckworth (1976), which includes a rather succinct review of temporally manipulated signals in aphasia research.

Marckworth's findings are not as clear cut as most of the work reported above, perhaps because of the expansion technique, or perhaps because the experimenter is more scrupulous statistically, and some subjects even showed degenerate performance on some of the expanded versions. But comprehension did improve as a function of signal duration, and the results are more encouraging than those of Liles and Brookshire (1975). The between-treatment variance was significant with a probability no greater than 0.05, and all but one subject (whose score was identical under both conditions) performed better with the temporally expanded signals. Further, the ambiguous results are mostly confined to the easier sections of the test. The harder the task, the more benefit is provided by increasing signal duration. This is clear from Table 2.3.

So the results, while not wholly uniform, are consonant with the findings reported above. As Marckworth says, they

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Table 2.3

RESULTS OF MARCKWORTH (1976)

Subjects' performance	Token Test subsections (increasing complexity →) I	II	III	IV	V
No Difference	4	4	3	2	3
Degraded performance	4	3	1	0	2
Enhanced performance	4	5	8	10	7

Number of subjects by performance on Token Test subsections I-V. (Taken from p. 89.)

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demonstrate that "temporal expansion of the natural language signal does facilitate improved comprehension in at least the subtype of aphasic patient tested" (90) – and the high proportion of expressively disabled subjects correlates her pool with the Broca patients this thesis is primarily concerned with.

Blanchard and Prescott (1980)

A more recent study, employing the Revised Token Test (McNeil and Prescott [1978]), has produced essentially the same results, but its authors view it as "at variance with the findings of other researchers". Blanchard and Prescott (1980; quotation p.120) presented their stimulus sentences under four conditions: the control rate of 150 wpm, and three expansions of that signal by factors of 1.25, 1.50, and 1.75. Accuracy of response was measured and, as expected, correctness was both greater and far less variable for the control group (23; neurologically sound) in all conditions. And, as predicted by earlier work, aphasic comprehension improved with signal expansion. One mild surprise, however, was that the improvement was not linear: the test "means for aphasic subjects [23; above the 50th% on the PICA] were 12.59 at the no expansion factor; 12.76 at the expansion factor 1.25; 12.91 at the expansion factor 1.50; and 12.84 at the expansion factor 1.75" (119).

These differences look slight, but they are hardly at variance with earlier findings. The range of these means is 0.32, which contrasts significantly with the 0.03 control group range. More to the point, the results of Parkhurst (1970), Liles and Brookshire (1975), and Marckworth (1976) do not show across the board improvement. Expansion does not facilitate comprehension for all aphasics – there are substantial differences in degree and quality among aphasia deficits – and a *post hoc* factor analysis of Blanchard and Prescott's (1980) data uncovers a subgroup of 15 aphasia patients who evidence a marked increase of comprehension as a function of signal expansion: significant at the $p<0.01$ level of confidence.

Poeck and Pietron (1981)

The largest study to date of a temporally manipulated Token Test is reported in a brief note by Poeck and Pietron (1981). They tested 42 aphasics (mostly non-fluent global patients, but not subtyped) and 42 right-damaged nonaphasics, at two rates of presentation: normal, and that signal expanded by a factor of 1.25. They found (1) there was a learning effect for the control group, but none for the aphasics; and (2) the aphasia victims' performance improved significantly in the longer condition (α=0.01), while the controls responded no differently as a function of duration. The second result accords with all the other work reported in this review, and offers yet further evidence that aphasia involves some temporally dependent deficit. The first offers less direct support, by demonstrating the Token Test's power:

> the fact that even an immediate representation of the test – although in two acoustically different versions – did not lead to a learning process underscores the great specificity of the TT for tapping one or more basic factors in aphasia.

The experimental results of temporally altered Token Tests are all the more compelling when this absence of a learning effect is considered.

Riensche, *et al.* (1983)

Another, more recent study – Riensche, Wohlert, and Porch (1983) – also claims, like Blanchard and Prescott (1980), to be at variance with earlier work; again, this is not quite the case. They administered the Revised Token Test to 15 subjects (mean PICA score=11;43) over four trials at four different rates of presentation: a base rate of 150 wpm, and expanded versions of 30%, 60%, and 100% more duration. The authors found no statistically important differences among these conditions and observed that the

> lack of a significant effect for rate upon comprehension scores ... is not particularly supportive of aphasics' improved performance by presenting speech at specified, unvaried TE [time-expanded] rates (46).

But the missing significance appears to be more a product of analysis than data. Riensche *et al.* only compared the performance averages over all subsections of their test: Parkhurst (1970) and Marckworth (1976) show that expansion is primarily effective in the difficult sections of the Token Test, a fact that would be obscured by averaging. Yet even working with overall averages, 11 of their 15 subjects had optimal comprehension rates in the expanded conditions. Further, of the four who performed best at 150 wpm, three had that rate as their last trial (43), and while there was no significant trial effect, a slight improving trend is evident from the trial means ($\bar{x}_1$=10.66, $\bar{x}_2$=10.73, $\bar{x}_3$=10.90, and $\bar{x}_4$=11.02).

Summary: Brookshire and Nicholas (1984b)

Riensche *et al.* (1983) certainly have reason to be circumspect and to characterize their results with phrases like "not particularly supportive"; still, the study is not at radical odds with earlier research into aphasic comprehension of temporally expanded speech. Their study shows inconsistency — some improvement, some absence of change, even some regression. The earlier studies show inconsistency. And the significance findings between studies are inconsistent: most uncovered significant improvement (Sheehan *et al*. [1973], Garder *et al*. [1975], Weidner and Lasky [1976], Lasky *et al*. [1976], Goldfarb and Halpern [1981], Poeck and Pietron [1981], Pashek and Brookshire [1982]); but some reported ambiguity (Parkhurst [1970], Liles and Brookshire [1975], Marckworth [1976]); while others reported no meaningful significant differences (Blanchard and Prescott [1980], Riensche *et al*. [1983]). This inconsistency is the subject of Brookshire and Nicholas' (1984b) study. They administered the Revised Token Test to 10 aphasic subjects, employing three distinct temporal conditions and two levels of difficulty. More to the point, they tested each subject four times, one week apart, and attempted to locate some consistency in subject performance in correlation with three important aphasia variables. They found no consistency at all.

The four trials consisted of 10 Low Error and 10 High Error commands, each in the following conditions: a Standard condition, of 200wpm, with natural pauses; a Slow Rate, of 100wpm, with natural pauses; and a Pause condition, of 200wpm, with one 4 second pause inserted at a word boundary in the command. The order of presentation was controlled such that the commands were randomized within level and condition, the conditions were randomized within each trial, and the levels of difficulty were reversed between the first two and the last two trials. The patients did not perform reliably, either as a group or individually. Some improved as a function of expansion, some degraded with expansion, and some had improved performance for one condition and one level on one occasion and degraded performance for the same combination on another trial. Moreover, Brookshire and Nicholas correlated performance levels with severity, postonset time, and type of aphasia, and no correlation reached significance ($p<0.05$). These findings are quite cloudy, even by the necessarily relaxed standards of aphasiology, and while they do not vitiate the earlier temporal studies, they do indicate

> previous reports of the effects of pauses and slow rate upon aphasic listeners' comprehension should be interpreted with caution (327).

However, there is good reason to treat Brookshire and Nicholas (1984b) with some caution as well. For one thing, the sessions may have been quite taxing (60 demanded responses: 10 commands x 3 conditions x 2 levels). Since the presentation was randomized, this fact alone could account for inconsistent results: if the patients were systematically doing poorly in, say, the last third of the session, those performances would be distributed over all conditions, levels and trials. But their choice of location for the interpolated 4 sec pause also may not have been to the subjects' advantage. Recall that Liles and Brookshire (1975) found placing their (5 sec) silence early in the sentence improved performance significantly ($p<0.05$) over placement later in the sentence. Yet Brookshire and Nicholas (1984b) ignored that finding in their design and adopted a general strategy of interpolating "near the center of each command" (324). Further, the Low Error and High Error levels were established individually for each patient after a (live-voice, normal presentation) run of the RTT: a Low Error "subtest was one in which a subject made 2-4 errors (on 10 items) ... [a High Error] subtest was one on which a subject made 5-7 errors" (325). Given the highly variable nature of aphasic performance (a fact documented by Brookshire and Nicholas), setting the difficulty levels from one session — rather than from the average of several sessions, or by arbitrarily choosing two RTT subtests for all subjects — is a questionable decision.

But the really problematic factor in the experiment was the extreme heterogeneity of its pool. The subjects ranged in age from 46 to 86 years old, in postonset time from 1 month to 10 years, in RTT pretest percentile ranking from 1 to 95, in PICA percentile from 36 to 81, and consisted of three syndrome types (4 Broca's, 2 Wernicke's, and 4 conduction). While this diversity is required for the consistency questions the experiment attempts to answer, it is nonetheless troublesome in so small a pool. The possibility of interaction is too great to ignore, yet the paper never mentions it. Consider just the Broca group. Of the four subjects, one was 50 years old, 10 months postonset, in the 1st RTT percentile, and the 43rd PICA percentile; another was 51, one month postonset, in the 10th RTT percentile, and the 36th PICA percentile; a third was 46, five years postonset, and in the 79th percentile for both the RTT and the PICA; the fourth was 55, three years postonset, in the 95th RTT percentile, and the 50th PICA percentile. Only age is relatively constant within the group. Looking for reliability in such multivariate circumstances with only 10 subjects is perhaps too optimistic.

Nevertheless, despite a lack of statistically significant consistency and a multiplicity of variables, Brookshire and Nicholas (1984b) do uncover a trend (apparent from tabulated data but not commented upon): the most systematic result is an improvement of comprehension relative to the Standard condition, under both manipulations, for both difficulty levels. Of the 160 difference scores they generated (tabled on 326), 108 show improvement, and 57% of those are significant (α=0.05). Only 10 difference scores indicate significant degradation of performance relative to the Standard condition, eight of which are the responses of two subjects and are quite systematic (for one subject, all four scores are in the Low Error/Pause condition; for the other, all are High Error and three are Pause).

In short, Brookshire and Nicholas (1984b) constitutes a synecdoche for all the temporal work reviewed above: little of it produces uniform results; all of it uncovers improved performance as a function of increased duration, to a greater or lesser extent. The other temporally manipulated comprehension studies are statistically more convincing on this second point than their Token Test counterparts – none of them failed to find significant improvement – but the generally more lax designs tend to compromise the statistics. The result is not remarkably robust, but it is there all the same: temporal expansion facilitates aphasic comprehension.

2.1.4 Aphasic Perception of Stress

Goodglass *et al*. (1967)

The earliest paper to examine systematically the role of stress in aphasia – Goodglass, Fodor, and Schulhoff (1967) – was interested in precisely the issues that motivate this thesis: the interaction of stress and function words in Broca's aphasia. Unfortunately, they did not test comprehension directly, but their work concerns peripheral processing mechanisms that impinge upon comprehension. They designed a number of simple repetition tasks controlling for rhythm and minimally implicating formal linguistic skills.

Drawing on two aphasic populations, fluent and nonfluent, in various combinations over several tasks, they found the clinical division to hold up experimentally. The groups had markedly different performance levels. The principal finding was that initial unstressed function words of three-word clauses were the most difficult for Broca's aphasics, but were quite easy for the Wernicke's. Further, there is some evidence that stress and not word class was the contributing factor, at least for the Broca victims.

First, the nonfluents demonstrated equal facility in repeating random three-word lists, independent of whether the items were content words or functors. Second, the best retained prosodic pattern was ´ ˘ ´, again irrespective of word class; in fact the tokens with initial functors were reproduced marginally better than those with initial content words ("Arén't thěy huńgry" vs. "Dógs căn bárk"). The next best pattern (´ ´ ´) also contained an initial function word. The only crucial test remaining – an unstressed initial content word – is unavailable in English, and could not be tested. But initial unstressed function words were the worst condition and initial stressed function words were the best condition, with a significance probability beyond 0.01, and there was no significant difference between retention of stressed functors and stressed content words. Stress (or salience) is a far more likely candidate than word class. (The Wernicke's group showed significant interaction with prosody as well, and their performance was neither inverse nor parallel to the Broca's group. Stress clearly plays some nontrivial role in their language disability, but one that is outside the province of this thesis.)

Kellar (1978)

Kellar (1978) is more of a theoretical paper than an experimental one. Although it centres on a replication of Zurif, Caramazza, and Myerson (1972), it adds little to their findings.[10] Zurif *et al.* (1972) observed that when Broca's aphasics were required to choose the words of a sentence (in pairs from random arrays of three) that 'go best together', the tendency was to group content words with one another and function words with one another. On the other hand, the normal subjects did just the opposite: they grouped words into phrasal constituents. For instance, with stimulus sentence 8, the strongest preference of Broca victims is to group *lawns* and *mowed* together; the strongest preference of normals is to pair *the* and *lawns*.

8. The lawns were mowed.

Kellar found exactly the same tendencies in her replication, but she expanded the subject pool to include Wernicke's aphasics and found that they also tend to group content words with content words and functors with functors. This is the only new empirical result of her work; the paper's merit lies elsewhere.

Zurif *et al.* (1972), and a plethora of subsequent papers by the same principals (discussed in some detail below), all hold this grouping tendency to evidence a specifically syntactic deficit. But Kellar (1978) notes that functors are not stressed sententially, while content words are, and suggests the aphasic is following "a simple heuristic: 'Whenever possible, cluster stressed words together'" (2). This interpretation of the data is at least as likely as the Zurif-Caramazza proposal. However, while her proposal is certainly tenable, and in some respects more attractive, it hardly amounts to a disconfirmation of the syntactic hypothesis. For the moment, neither is a clear winner.

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[10] Kellar's paper was delivered to the Academy of Aphasia, and has not been published. My principal access to her argument is by way of an abstract sent to me by Dr. Edgar Zurif, along with the enigmatic warning: "the work is somewhat problematic". It is also discussed briefly by Brown (1980, 1982), and Berndt, Caramazza and Zurif (1983).

Laughlin *et al.* (1979)

Laughlin, Naeser,and Gordon (1979) provides another interface of sorts, this time between the temporal work of the previous section and the experimental manipulations of stress considered below. It accommodates this role for two reasons. First, the authors employ duration specifically as "the single most important factor in perception of stress in English" (312). Second, they control the temporal feature of syllables in an implementation of Melodic Intonation Therapy (Sparks, Helm, and Albert [1974], Sparks and Holland [1976], Sparks [1981]).

Melodic Intonation Therapy is a clinical program to treat the expressive difficulties of the nonfluent aphasias. It is comprised of five successive treatment levels, all of which pay very close attention to the suprasegmental characteristics of speech. First the therapist hums the intonational pattern of an utterance, requiring the patient to hum along. Then the utterance, retaining stylized intonation, is spoken in unison by therapist and patient. This continues, with the clinician gradually participating less, and the aphasic more, until the patient can produce intoned utterances independently. Laughlin *et al.* (1979) administered this therapy to five nonfluent aphasia victims (2 global, 2 mixed, 1 Broca's) under three conditions of syllable duration: (1) a 'normal' period of about 300 msec; (2) an expanded period of about 1.5 sec; and (3) a further expanded period of about two sec. (Times are approximate because, due to the necessarily flexible nature of the sessions – frequently requiring repetitions and returns to earlier stages of therapy – presentation was "with live voice" [315].) As Table 2.4 indicates, the results were dramatic.

Table 2.4

RESULTS OF LAUGHLIN *ET AL*. (1979).

	Normal				1.5 sec					2 sec				
	MIT Steps				MIT Steps					MIT Steps				
	2	3	4	5	1	2	3	4	5	1	2	3	4	5
Subjects														
Global	4	4	4	3	20	12	11	9	8	20	12	12	11	10
Global	8	8	6	6	20	13	11	11	10	20	17	17	17	17
Mixed	1	1	1	1	20	20	19	14	13	20	19	16	16	16
Broca's	3	3	3	3	20	20	20	17	14	20	20	20	19	19
Mixed	6	6	6	6	20	20	20	13	12	20	20	20	20	20

(Taken from p. 316; 20 is the highest possible score for any section.)

The order of the condition presentations "was counterbalanced" (316), which presumably means that each subject did not receive therapy in the same order for the three conditions. And while the most impressive improvements may have been partially a learning effect, it is clear that performance is enhanced by increased duration, across the board. The differences between the normal and the 1.5 sec condition proved statistically significant at $p<0.005$; between normal and two sec durations, improvement was significant at $p<0.001$; but no interesting variance was observed between the two expanded conditions.

This study says nothing specific about comprehension but, like Goodglass *et al.* (1967), it does address an essential prerequisite to comprehension: perception. It demonstrates that nonfluent aphasics can perceive suprasegmental properties, such as "melodic line, rhythm, and points of stress" (311), and that this perception is facilitated by temporal expansion.

Swinney *et al.* (1980)

A more clearly experimental demonstration of stress perception in nonfluent aphasia is provided by the study of Swinney, Zurif, and Cutler (1980). Their paradigm affords the standard incontovertible experimental evidence of perception: a manipulated variable that results in a statistically interesting effect. They asked eight Broca's aphasics to push a button upon hearing a target word (e.g., *ball*) embedded in a sentence controlled for stress along the following lines:

9 a. The umpire said a new báll was necessary.
b. The umpire said a néw ball was necessary.

That is, in one token the target word received primary sentential stress; in the other, a nearby word received primary stress, necessarily reducing the target word. No subject heard both tokens of a sentence type. Response latencies were measured, and Broca victims reacted more quickly to those targets carrying stress than to those without it ($\bar{x}$=489.5 msec vs. 566.5 msec; significant at α=0.025). A significant effect was observed, and perception was demonstrated. More interestingly, since the above studies also revealed stress perception, Swinney *et al.* (1980) also showed that this latency pattern is present in neurologically sound subjects, though at a higher significance level (α=0.05) and with faster times overall. So, not only do Broca's aphasics perceive sentential stress, it facilitates lexical identification for them in roughly the same fashion it facilitates normal lexical decisions.

2.1.5 Stress and Aphasic Comprehension

Blumstein and Goodglass (1972)

Harold Goodglass' second venture into aphasic stress comprehension, Blumstein and Goodglass (1972), had the distinct benefit of a phonetician riding shotgun, but focusses specifically on lexical stress and consequently is only of indirect interest here. They tested a group of 17 aphasics on a picture-matching task. The stimulus set was comprised of lexical pairs distinctive solely by stress pattern (e.g., "sórehead" and "sore héad"). The matched items were chosen from an array of four conventional pictures, two of which depicted the conflicting meanings and the other two were distractors – e.g., pictures of (1) a boy, frowning, with a darkened 'thought ballon' over his head ("sorehead"); (2) a boy, grimacing in pain, with a bandage and a lump on his head ("sore head"); (3) a boy crying (lexical distractor for "sorehead"); and (4) a foot with illustrative symbols of pain 'radiating' out (lexical distractor for "sore head").

The aphasia victims made more stress errors (e.g., choosing 2 as a match for "sorehead") than 'random' errors (e.g., choosing 4 as a match to "sorehead"), and of course significantly more errors overall than a neurologically sound control group ($p<0.025$). But the between-error type variance proved statistically uninteresting. Stress did not cause the aphasics any specific trouble; precisely the expected result if, as Goodglass *et al*. (1967) indicates, aphasia does not significantly affect the victims' ability to perceive stress. Blumstein and Goodglass (1972) consider stress in their experiment to be a semantic cue, when a syntactic analysis is equally likely, but they draw the obvious conclusion: "the perception of stress is preserved in aphasia" (806).

Baum *et al*. (1982)

Baum, Daniloff, and Daniloff (1982) pursued the answers to three clearly formulated questions, two of which are of immediate consequence to this thesis:

> [1] Can Broca's aphasics utilize lexical stress to disambiguate otherwise homonymous sentences? ... [2] Does raising the stress level of functor words in sentences increase their retrievability by Broca's aphasics? (264)

Unfortunately, the answers afforded by their paradigms are of no consequence whatsoever.

The answer they provide for [1] is a qualified "no". They arrive at this from the results of a sentence-picture matching task predicated on sentence pairs of the following sort:

10 a. That's a *hot* dog.
   b. That's a hot *dog*.

11 a. She is home *sick*.
   b. She is *home* sick.

(269; italicization represents primary stress)

Eight Broca victims and eight neurologically sound controls were required to select the correct picture for a given sentence from a vertical array of three, which included a depiction of each competing signification and a distractor. Not surprisingly, aphasic performance was significantly worse on the task, with an α-level of 0.001. Ergo, conclude Baum *et al.*, "comprehension mediated by suprasegmental cues is somewhat impaired in this group of Broca's aphasics" (268). But there was insufficient experimental control to support even this modest conclusion. For instance, only one level of stress was used, and only one sentence type. If there was no interesting variance in performance on normal vs. emphatic stress for sentences like 10 and 11, or if Broca victims fared the same on lexically ambiguous sentences, the authors would have some substance for their claim. Similarly, if they had tested another aphasic subtype, or right-damaged patients, their conclusion could be given more credence. But aphasia victims, even mild expressive ones, will score worse on virtually any comprehension task, and a number of nonlinguistic variables are likely to be responsible; fatigue, inattention, or emotional difficulties, for example. There is no support for their contention that Broca's aphasics have a particularly suprasegmental impairment.

The answer [2] receives is less compelling yet. Baum *et al.* report "the failure of emphatic stress to enhance aphasics recall of functors" and suggest this supports the argument "that stress is not a salient feature in explaining retention and loss in Broca's aphasia" (269). But the functor test appears to have been very poorly designed: the only stimulus sentences they provide in the paper are the sentences of 12.

12 a. The spoon is in the glass.
   b. The spoon is *in* the glass.

(264; italicization represents emphatic stress)

That is, although two conditions were presented, the stimuli were perfectly transparent, irrespective of condition, irrespective even of the functors; as discussed in connection with the Token Test above, pragmatically obvious spatial relations can be resolved simply by understanding the content words. In fact, the subjects did very well on this task – 82% correct, as against 56% for the first task (significant at $p<0.001$), and even the controls found it easier – which indicates most of the sentences were at least as obvious as 12.

While the statistical analyses of Baum, Daniloff, and Daniloff (1982) support the claim that Broca victims are peculiarly insensitive to stress, their experimental methodology does not.

Pashek and Brookshire (1982)

Using the subjects, methodology, and stimulus set outlined in the temporal section above, Pashek and Brookshire (1982) found counter-evidence to the claims of Baum *et al*. The control stimuli were read with "'normal' stress and intonation" and were compared with a reading "in which facts were given extra prosodic emphasis (e.g., Samuelson and Harbo were two *fishermen* who crossed the Atlantic Ocean alone)" (379). (They make no mention of whether the highlighted facts systematically corresponded to the yes/no questions used to assess comprehension, but their examples indicate that this was the case.) The findings were all but identical to the rate of presentation data reported earlier: there was no interesting nonaphasic variance, and aphasic performance was significantly better for the exaggerated stress condition than for normal stress ($p<0.01$).

Another, very intriguing, result also came out of these studies. In all, Pashek and Brookshire (1982) had 4 conditions: (1) slow rate, exaggerated stress; (2) slow rate, normal stress; (3) normal rate, exaggerated stress; and (4) normal rate, normal stress. As reported in the temporal section, 1 and 2 were significantly easier for aphasics as against 3 and 4; 1 and 3 also resulted in significantly fewer errors than 2 and 4. This means that condition 1 resulted in the best overall performance, while condition 4 resulted in the worst overall performance. But – and here is the curious part – they were best and worst respectively by only small margins. That is, the rate x stress interaction proved statistically insignificant.

A likely account for this is interference: since one of the components of stress is increased segment duration, it may well be the case that a temporal component is affecting performance in both situations. In effect, testing for a rate x stress interaction may be equivalent to testing a slow rate reader against a signal expander. Pashek and Brookshire (1982) briefly entertain this possibility, but rule it out on the basis of random sentence duration tests they conducted for corollary sentences in the different stress conditions. They found no interesting variance. There is a very good chance, however, that a similar test between words would produce significant figures; in particular, *fishermen* would probably be of markedly longer duration under the exaggerated stress condition, as perhaps functors adjacent to such stressed words would probably be shorter than under normal stress conditions.

2.1.6 Summary

Perhaps the most likely explanation for the Pashek and Brookshire results, however, is the operation of a ceiling effect; that is, decreased rate made the crucial items more salient, and exaggerated stress performed exactly the same function. So, the question remains: does stress facilitate comprehension because its increased temporal dimension provides for more processing time, or does increased duration facilitate comprehension because, like stress, it boosts the acoustic salience of the message.

2.1.7 Function Word Experiments

As early as Wernicke (1908) it was recognized that neurologically damaged people evidencing telegraphic, effortful, dysprosodic speech also have problems of speech perception, albeit on a subtler level than their productive difficulties:

> [in this disorder] there is almost invariably a certain inability to understand complicated constructions and finer differentiations of speech ... I am no longer of the opinion that in pure motor aphasia the ability to understand speech always remains unimpaired (quoted in Marshall [1982:399]).

In fact, discordant with his current depiction as a dogmatic locationist, there is evidence that Paul Broca was aware of the perceptual side of the disorder he identified (Caplan [1980:234]). Yet the syndrome is frequently discussed in linguistic literature, past and current, largely within the context of unimpaired comprehension. There is good reason for this: the victims are usually far more adept at informal conversation than posterior aphasics. Anterior aphasics' comprehension is *relatively* unimpaired, and for many victims it is only with quite finely-tuned measures, such as the Token Test, that they demonstrate any comprehension deficit at all. And, as above, the TT is very revealing. Even though Broca victims show comparatively fewer difficulties in understanding daily conversation, or passing routine appraisals, while Wernicke victims do poorly at both, the Token Test cannot discriminate between the two dysfunctions (Orgrass and Poeck [1966], Poeck *et al*. [1972]). The two aphasias involve quantitatively similar perceptual deficits on specialized language tests.

Clearly, then, the disorders are different, qualitatively. Wernicke patients most probably score poorly on the Token Test for the same reasons they do poorly understanding a question like "What kind of work did you do before you became ill?". On the other hand, Broca patients have little trouble with such questions; so their performance on the TT must reflect another type of impairment. A good deal of quite recent work investigates the proposition that the nature of this qualitative difference centres on function words.

In contrast to the temporal and stress experimental work, there is a common theoretical thread running through the function word studies – initially proposed by Goodglass

(1968), but associated most closely with Edgar Zurif and, to a lesser extent, Alfonso Caramazza. Zurif, Caramazza, and Myerson (1972) claim to have uncovered a differential treatment of functors and content words by Broca's aphasics in nonproduction tasks. Since the principal symptom of Broca's syndrome is agrammatism – the omission of functors and inflectional morphemes in speech – this finding lends support to a competence analysis of the disorder. Specifically, if Broca victims treat functors analogously in production and perception, that argues for a 'higher order' disruption: some level underlying the performance of production and the performance of comprehension looks to be implicated.

In the wake of Zurif *et al*. (1972), a number of papers from the same principals (Caramazza and Zurif [1976], Zurif *et al*. [1976], Caramazza *et al*. [1978], Zurif and Blumstein [1978], Berndt and Caramazza [1980], Zurif [1980], Zurif [1982b], and Berndt *et al*. [1983]) have advanced the same general thesis: "syntactic processing is disrupted in Broca's aphasia" (Zurif and Caramazza [1976:290]). The result of this syntactic deficit, hampered access to function words, is that comprehension proceeds by way of pragmatic strategies which analyze only the content words of a sentence:

> Broca's aphasics understand a sentence primarily by inferring what makes factual sense from a sampling of the major lexical items – its nouns and verbs – independent of syntactic structure (Zurif [1980:307]).

However, while all the work reviewed below addresses this syntactic hypothesis, and most takes Zurif's research as a starting point, not all of it endorses the proposal, and the majority of results conflict with it.

Goodglass *et al*. (1970)

The first suggestion that Broca patients' comprehension problems might mirror their production difficulties appears in Goodglass (1968), who gives this hypothetical dysfunction the very apt label of *conceptual agrammatism*. Two years later, along with Gleason and Hyde, he reports on an attempt to test his proposal by running two function word experiments with a large aphasic subject pool (52: 7 conduction, 10 Wernicke's, 10 global, 14 Broca's, and 18 anomic); the hypothesis was not supported. Both tasks involved prepositions, and both were very straightforward. In the Prepositional Preference test, the subjects were simply asked to select the grammatical sentence, matching a picture (e.g., of a man leaning against a lamp post), from two alternatives such as 13.

13 a. He is waiting at the corner.
b. He is waiting to the corner. (603)

The second test, of Directional Prepositions, required the subjects to correctly select from an array of three the picture corresponding to a sentence read out loud to them. All pictures (18) invloved "one or two girls placed in proximity to one or two cars" (589), and the sentences were of the form given in 14.

14. Show me the girl behind the car.

Conceptual agrammatism, which claims Broca's syndrome entails a peculiar function word deficit, predicts both tasks to be problematic for Broca victims. As Table 2.5 indicates, the results do not support this position.

Table 2.5

RESULTS OF GOODGLASS *ET AL.* (1970)

	Directional Preps.		Prep. Preference	
Perfect Scores	24		10	
Subject Scores	$\bar{x}$	$\bar{x}^{+}$	$\bar{x}$	$\bar{x}^{+}$
Anomic	22.3	27.2	10.2	9.8
Broca	20.82	21.5	9.39	9.1
Conduction	20.0	18.7	10.7	10.2
Global	13.7		5.35	
Wernicke	17.47	19.8	6.4	7.9

Where $\bar{x}$ is the observed mean; $\bar{x}^{+}$ is covariance adjusted mean. (Taken from pp. 600-1.)

The figures of Table 2.5 were subjected to statistical significance tests; however, the reporting of these tests is confused. In particular, the discussion of the directional (or, more accurately, spatial) comprehension subtest does not align with the tabled data. Either 2.5 or the following paragraph is in error.

> Directional Prepositions are the least discriminating of the four tests [as revealed by analysis of covariance], with a probability of 0.09 that the differences among the means are due to chance. Here, the conduction aphasics score worst, but only by a margin of 2.8 out of a

> possible 24 from the highest scoring
> group – the Broca's aphasics (601).

Table 2.5 has the anomic patients as the "highest scoring group", not the Broca's. The conduction patients are indeed the worst, but separated from the highest group by 8.5 (though from the Broca mean by the reported 2.8). Since the spatial comprehension scores have a ceiling of 24, the tabled value of $\bar{x}'$ for the anomics is clearly inaccurate, and probably responsible for most of the confusion, but no simple error is in evidence (e.g., a figure of 22.7 would still falsify the statement that the Broca group performed best; a figure of 17.2 would falsify the statement that the conduction group fared worst). In any event, this statistical haggling is well beside the point, as Bartlett's test reveals homogeneity of variance not to be supported, either for the Directional Preposition or Prepositional Preference data. That is, ANCOVA is not a legitimate technique for the experimental results. The significance findings, whatever they might be, are valueless.

However, a rough and ready eyeballing of Table 2.5 suggests that nothing special is going on within the Broca subject pool with respect to either grammaticality judgements or spatial comprehension as a function of the tested prepositions. Further, Goodglass *et al*. (1970:600) supply standard deviations for the observed means, and in both cases the Broca SDs are relatively low (for directional comprehension it is the lowest, 2.49; for grammaticality judgement it is 2.09, less than the median in a range of

0.49 -6 .49). So the Broca results are not skewed. Eyeballing is sufficient here, and the prepositional experiments do not support conceptual agrammatism. Subsequent authors, arguing for a functor deficit in Broca's aphasia, have commented on Goodglass *et al*. (1970) and discounted the directional test by way of a "subtle constraint on the patient's performance" (Berndt and Caramazza [1980:239]); namely, that one of the NPs (*the girl(s)*) was animate, while the other (*the car(s)*) was not, which is said to have interfered by somehow cuing pragmatic strategies. This objection may be justified, though the reasoning is not transparent, but if so it only mitigates one of the findings, and no similar dismissal has been advanced for the grammaticality judgement subtest, which also indicates there is no peculiar functor deficit specific to Broca's syndrome.

Zurif *et al*. (1972)

The first clear indication that Broca victims may have a peculiar comprehension disorder involving functors comes from Zurif, Caramazza, and Myerson (1972; see also Caramazza and Zurif [1976]). They simply asked their small subject pool (3 Broca patients) to choose pairs of words from triadic arrays, matching them solely on the criterion 'goes best together'. A simple sentence including all three words was kept in view while the subjects chose. The sentences were of the following sort:

15. The dog chases a cat.

16. The girl was taller. (409)

They also had a neurologically intact control group perform the same task, and the data were subjected to a hierarchical clustering analysis. The clusters highlight two distinct tendencies. The control group tended to conjoin words according to phrasal constituency, putting words like *the* with *dog* and *a* with *cat*; their cluster charts look much like conventional phrase markers. But the Broca patients appeared to group words according to a function / content distinction, pairing *the* with *a* and *dog* with *cat*.

The statistical methodology is sound enough – a graph is derived for each sentence that expresses the frequency with which one word is grouped with another – but as Kellar (1978:2) points out, the experimental methodology is wanting in several respects. In particular, the population size and the lack of another aphasic subtype casts some doubt on the paper's claim of "isolating the syntactic impairment" (415) of Broca's syndrome. Further, the results were not wholly uniform. In the sentence given here as 17, the strongest tendency among the Broca patients was to group *my* with *shoes* (forming a conventional NP), next to match *where* with either of those words, and the weakest trend was to associate the verb with any of the other items.

17. Where are my shoes?

In short, a simple content / function strategy does not account for all the data.[11] Nevertheless, Zurif *et*

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[11] Cf. also Kolk and Van Grunsven (1984), reviewed below.

al. (1972) has been quite influential, and has motivated a number of better controlled investigations into the same questions.

Smith (1974a)

Zurif, Caramazza, and Myerson (1972) has also motivated a number of experiments without better controls, one of which is Smith (1974a). She reports findings that support the functor deficit hypothesis, but that involve too many loose variables to allow for adequate evaluation.

She ran two experiments with five aphasic subjects. One involved the manipulation upon request of household items randomly arrayed before the subject. She supplies four examples of these requests, given here as 18 - 21.

18. Put the coin in the bowl.
19. Point only to the key.
20. Touch the cup after the ribbon.
21. Put the pencil in front of the ring. (578-9)

The subjects were scored independently for comprehension of nominals and prepositions. The second experiment called for the reorganization of a random array of individual words printed on cards into target prepositional sentences. Each rearrangement trial was performed in view of real world objects placed in the spatial relation corresponding to the target sentence (e.g., a cup on top of a book would be the illustration for a randomized presentation of the words *the*, *cup*, *is*, *on*, *the*, and *book*).

Two of the subjects found both tasks particularly difficult, and both had the classic symptom profile of Broca patients (the others appear to have been Wernicke patients, one with pronounced anomia; see p.378). On the first task they had relatively little trouble recognizing and processing the named items (i.e., they were good with the nouns), but "were significantly impaired in the understanding of relational words [prepositions]" (379; $\alpha=0.01$); they usually selected the correct articles from the array, but less reliably performed the requested manipulations. None of the other subjects distinguished the nouns and prepositions in any statistically meaningful way (though the anomic patient was somewhat better with relations than items). On the second task, the two Broca subjects, along with the anomic patient, "produced what at first seemed like gibberish" (381). However, upon closer analysis, the sequences proved "highly systematic" (381). Further, Smith notes, this systematicity was guided by English word order strategies. She offers no examples of this tendency, except to say that one of the subjects with a Broca profile "always ended his statements with a noun" (382), but the problems appear to have been more with functor-content word configurations than subject, verb, and object placement. The other two patients performed the task with much less difficulty.

Despite the small number of participants, and the smaller number of Broca patients, in concert with Zurif *et al.*'s (1972) results Smith's work seems to add robustness to the functor deficit hypothesis. Unfortunately, there were too many other factors at play. First, there is the issue of pragmatic interference (raised above with respect to the Token Test, and Baum *et al.*): putting a coin in a bowl is a fairly predictable manipulation, even if only the words *coin* and *bowl* are understood. Second, there is a problem with meaningless prepositions. In the case of 19, *to* is completely redundant - it could, for instance, as easily have been *at* - and the appropriate action is sufficiently cued by the linear order of *point* and *key* alone. Third, some of the stimuli apparently contained no prepositions at all (*after* in 20 is an adverbial subordinator, since its 'object' is the elided gerundive *touching the ribbon*, not a NP). Fourth, some of the stimulus requests involved compound prepositions (such as 21's *in front of*), which provides for sonic interference. *In front of* carries sentential stress, and is substantially longer than the prepositions of 18 and 19, *in* and *to*. As the studies reviewed in previous sections demonstrate, both stress and duration affect comprehension.

There is one feature of this interference which might suggest support of the Zurif hypothesis, however. Most of the factors just outlined should act to improve prepositional performance, not retard it, yet Broca subjects still did worse with functors than substantives. An argument might be advanced that a function word deficit would have to be very severe in order not to benefit from pragmatic, redundant, stress, and temporal aids. Further, the second task is not greatly compromised by these lapses in control. But most probably these were not the only problems of experimental design; when only four stimulus examples are given, and each of them indicates a distinct flaw in the design, it is difficult to have confidence that the stimuli not supplied are better controlled. In any event, when independent variables are allowed to roam free in an experiment, simple linear results (such as improved performance) cannot necessarily be expected. If there is support anywhere here for a functor deficit in Broca's aphasia, its scope is severely limited.

Heilman and Scholes (1976)

Explicitly taking their lead from Zurif *et al.*'s (1972) suggestion of a "linguistic incompetence" entailed by Broca's syndrome, and "dependent on syntax rather than major lexical items" (259), Heilman and Scholes (1976; also reported in Scholes [1978]) devised an elegant little comprehension test which places an uncharacteristically heavy information load on *the*. They also expanded the subject pool to include 26 aphasics, of three equally distributed subtypes (Broca's, Wernicke's, and conduction), and a control group of neurologically damaged nonaphasics; thereby sidestepping a principal flaw of the earlier paper. The task was a simple picture-matching paradigm: the subjects were read a sentence and were required to pair it with a line drawing which depicted the described action. Sentences were of the type illustrated by 22.

22 a. The man showed her baby the pictures.
b. The man showed her the baby pictures.

That is, the meaning distinction rested solely on placement of *the* in the verb phrase (presumably intonation was controlled for). The line drawings were presented in groups of four, depicting, for example:

23 a. A man showing a baby some pictures.
b. A man showing a woman some baby pictures.
c. A man showing horse shoes to some boys.
d. A man showing a horse some boy's shoes.

So each trial had four potential responses, one correct and three erroneous, and there was a qualitative difference among the wrong choices. If, for instance, a subject matched 22a with 23b, the error would implicate the definite functor. This choice could be accounted for by the perception of 22a as 22b, or perhaps as the ambiguous 22c.

22 c. The man showed her baby pictures.

On the other hand, matching 22a with 23c or 23d suggests a fundamentally different mistake, implicating at the very least the semantics of *woman*, *baby*, *picture*, *horse*, *boys*, and *shoes*. Heilman and Scholes (1976) scored errors of the first type as *syntactic*, and errors of the second type as *lexical*, and while the labels are not as self-evidently appropriate as they might hope, the differentiation is patently valid.

The Wernicke patients made significantly more errors of both types (p<0.01) than each of the other groups. In fact, their performance on the task was more or less random ("not significantly different than chance" [262]), supporting the contention that the difficulties they show with bedside questions and requests carries over to more specific types of investigation. The remaining three groups produced no interesting variance for lexical mismatches – very few were committed – but the Broca and conduction pools made significantly more syntactic errors than the nonaphasics. Their performance was also close to chance level, though only for the restricted subset of responses hinging on determiner placement (e.g., 23a and 23b). They appeared to perceive 22a and 22b as ambiguous (=22c), attending only to major lexical items.

Heilman and Scholes (1976) use these data to advance the claim that Broca patients have a processing disorder of peculiarly syntactic tenor. Scholes (1978:183) is slightly balder: "impairment of syntactic comprehension is the crucial point". Clearly the matter is still open for debate, but there is a more curious aspect to these papers. They both downplay the fact that Broca and conduction victims have comprehension patterns of the same order, at least as measured by the above paradigm (no significant difference was found between the two groups in any response pattern). Yet this fact does not threaten the syntactic incompetence hypothesis, or the locationist argument it entails (centreing a syntax module in Broca's region) since conduction patients result from damage to the arcuate fasicules, a bundle of neural fibers which plugs into Broca's area. Indeed, the conduction group's performance offers indirect support of these positions by calling into question syntactic comprehension experiments that fail to discriminate between fluent and nonfluent subtypes (cf. the survey of Linebarger, Schwartz, and Saffran [1983]), as the fluent subject pool might well have included conduction patients who would bias the results.

These points aside, Heilman and Scholes (1976) is a very valuable study. The experiment was well designed, the subject population relatively broad, and the results important. They clearly isolate for the first time a significant aspect of the qualitative comprehension differences between Broca's and Wernicke's syndrome. The syntactic label they put on this difference – and the extent to which they generalize their findings – is suspect, but its existence is established.

Shewan (1976)

Evidence of the reverse sort, that Broca patients have no peculiar functor deficit, is reported by another article in the same volume of *Cortex*, Shewan (1976). She tested 27 aphasia patients (9 Broca's, 9 Wernicke's, and 9 amnesic) on the auditory comprehension test introduced in Shewan and Cantor (1971), and examined error patterns as a function of word class (noun, verb, adverb, adjective, pronoun, and preposition). No group was found to treat any word class with particular facility or incompetence.

The Shewan and Cantor (1971) test is poorly documented. Only a small class of stimulus sentences (8 of 42) is given, along with only one set of the line drawings (from 42 sets) used to evaluate those sentences. The overall design, though, is apparent. First, the subject is read a sentence on the order of 24.

24. The girl is reading a book.

The task is then to match this stimulus to one of four pictures representing, for example:

25 a. A girl reading a book.
b. A girl reading a newspaper.
c. A girl sleeping.
d. A boy reading a book.

The response set was drafted so that each incorrect representation (25 b - d) "differed from the correct representation in only one critical item" (Shewan and Cantor [1971:214]). No clarification is offered in either paper as to how the depictions might differ for a prepositional or pronominal critical item, and no corresponding stimulus sentences are provided, so it is not possible to judge Shewan's (1976) claim of equal impairment for all lexical items, content or functor, for all tested disorders (and of course not all function word categories were tested). Nevertheless, it is noteworthy that neither prepositions nor pronouns generated a significant effect crossed with the Broca subgroup, and the impressive (Kendall's) concordance statistic (W=0.936) suggests very close correspondences between subject groups.

Caramazza and Zurif (1976)

Carammazza and Zurif (1976) is a largely theoretical paper addressing the issue of whether algorithmic and heuristic processes of language comprehension are potentially dissociable; that is, whether constituent analysis (i.e., parsing), and semantico-pragmatic analyses are in principle psychologically distinct. A rather nice study to this end was designed and implemented with three aphasic subject groups. The results indicate that the two processes are dissociable, but also that Broca's syndrome involves some impairment of the former.

Using four sets of eight sentences, as illustrated by 26 - 29, they required 15 subjects (equally comprised of Broca's, Wernicke's, and conduction aphasics) to perform a forced choice picture matching task. Sentences were read "in a clear voice at a conversational pace" (578).

26. The girl is kicking a green ball.
27. The apple that the boy is eating is red.
28. The cat that the dog is biting is black.
29. The dog that the man is biting is black. (575)

Type 26 sentences were simply included as controls, subject distractors of a sort common to psychological investigation (but quite rare in experimental aphasiology). Like the remaining sentences, the controls include two underlying propositions (*the girl is kicking the ball* and *the ball is green*). Types 27 - 29 are all instances of centre-embedded relative clauses, and they vary systematically in terms of plausibility. Type 27 sentences are designed so that the (syntactically) correct reading of the embedded clause is also the most plausible (boys eat apples, but apples don't eat boys). Type 28 sentences are all semantically reversible (cats bite dogs and dogs bite cats), though perhaps some readings are favoured. The correct reading of type 29 sentences is the least plausible (dogs bite men but men rarely bite dogs). Only two pictures were used: a correct one and one that included both principals (e.g., a man and a dog), but depicted a false attribute, verbal action, or subject-object relation.

The Wernicke group, surprisingly, performed quite well across the board, though the authors surmise "we likely included only very mildly impaired, atypical Wernicke's aphasics" (579), due to the comprehension difficulties posed by centre-embedded relatives. The crucial point, however, is that they evidenced no sensitivity to any of the conditions; error rates were evenly distributed for all sentence types and for all picture types. In marked contrast, both Broca's and conduction aphasic responses varied significantly ($p<0.025$) as a function of sentence plausibility and erroneous depiction. Of special interest is their performance for all sentence types where the forced choice differed only in subject-object relations. With semantically constained type 27 sentences, their mean performance level was at 90% correct; with the reversible (28) sentences, the responses dropped to chance level; and with the implausible (29) sentences their scores were slightly lower ($\bar{x}$=40%).

Caramazza and Zurif (1976:581), as is their wont, conclude the Broca and conduction groups "are unable to use the syntactic-like algorithmic processes" and therefore Broca's area "subserves the syntactic component of both comprehension and production" (581). This seems a little hasty. If the Broca patients were relying solely on heuristic strategies, their %-correct rates would have a mean near 10; they would consistently interpret the dog as subject and the man as object in sentences like 29. The fact that their response rates are close to chance indicates they were confused about grammatical role assignment, not that they had no recourse to syntax. In fact, it could only have been syntax that kept their rates as high as they were. Caramazza and Zurif's results demonstrate that Broca (and conduction) patients rely more heavily on semantic strategies than syntactic ones, not that the latter are wholly lost. A reasonable alternative hypothesis, suggested by the Heilman and Scholes (1976) results, might be that at least some of the function words of 27 - 29 are not readily available to them. Such a deficiency would restrict the input to a parser enough to result in confusion, but would not disable it altogether.

Goodenough *et al*. (1977)

Another nice paradigm, based on the Token Test, was run by Goodenough, Zurif, and Weintraub (1977). The results are more questionable, due to lax control, but they support the findings reported above and with some reservations, contribute to the theory that a functor deficit of some order comprises part of Broca's aphasia. Goodenough *et al*. presented their subjects with three simple geometric shapes, in two colours: a white circle, a black circle, and a black square. The task involved direct response to elementary requests:

30. Touch the white one.
31. Touch the square.
32. Pick up the black circle.
33. Pick up a circle.

Interspersed with these appropriate requests were some quite odd ones:

34. Touch the black one.
35. Touch the circle.

These requests are patently inappropriate for the task at hand. Since there are two black objects, there is not an individual, *definite* referent for *the black one*; since there are two circles, there is no definite referent for *the circle*. (Presumably order of presentation was controlled to maintain inappropriateness; for instance, 34 is not problematic after "Touch the white circle".) Confronted with these circumstances, the neurologically sound control group ("three members of the laboratory personnel" [17]) adopted the following strategy: they restricted the response choices to a subset for which the request was appropriate and then carried out the action required. For 34 they restricted their choice to the set of circles, and chose the black one. For 35 they restricted their choice to the set of black objects and chose the circle. This strategy is evidenced by response latencies (e.g., a slower response to 34 than 30), and by choice (e.g., the black circle is touched for request 34).

Though this performance pattern is the expected one (cf. Olson [1970:264ff]), the unfortunately small and possibly biased control group makes somewhat suspect both extrapolations to a normal population and significance tests against the aphasics. The aphasia data are a little more useful, but only a little. Goodenough *et al.* (1977) tested four subgroups: Broca's (n=4), mixed anterior (5), Wernicke's (5), and anomic (4); the two anterior groups were agrammatic. Only the anomic group produced latencies and choices of the appropriate sort (i.e., the same as the neurologically sound group). Both groups of anteriors and the Wernicke group showed no latency differential and no distinctive error pattern; they responded at a rate of about 50%, touching either black object for 34 and either circle for 35. This is the pattern predicted by Heilman and Scholes' (1976) results, of course, since none of these subtypes demonstrate access to the fragile meaning difference cued by the inappropriate use of *the*. However, there is a confounding factor: the subjects were rated as either moderate or severe (no metric supplied), and the "severe group ... included only one Anomic aphasic, all the Mixed Anteriors, and the majority of the Broca's and Wernicke's" (18). In other words, the elegance of their paradigm was partially hamstrung by a lack of subject control, and their uncovered comprehension distinction is just as likely to be a product of severity as of syndrome. Still, the anteriors are defined pretest by "relatively intact comprehension" (12) while the Wernicke group is "characterized by severe comprehension problems" (11), so they are comprehensionally distinct in terms of informal evaluation. Therefore, since this pretest distinction collapsed with subtle differences cued by determiners, Goodenough *et al.* (1977) points to the same qualitative impairment difference noticed above.[12]

Caramazza *et al.* (1978)

Another Caramazza-Zurif study, this one with Howard Gardner (1978), provides some interesting data which compromise their functor deficit hypothesis. The task involved the recall of a target word upon hearing a probe, the word immediately preceding the target in a stimulus sentence. The sentences were read aloud and the probe was given after no delay and no distractor tasks. Length and syntax were controlled for as illustrated by 36 - 38: one class of sentences was comprised of five word actives (36); another, of six word passives (37); and the third set contained six word sentences with centre-embedded relatives (38).

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[12] Zurif (1982b:206f) reports on a replication of this study, using written stimuli and giving the subjects "unlimited time", which demonstrates that agrammatics can detect inappropriate article use in the right circumstances. That is, they retain the competence necessary to detect meaning cued by functors, but their performance is compromised, at least for oral stimuli.

36. The dog chased a cat.
37. The books were delivered by John.
38. The bike that John holds is broken.

The results were scored, for no explained reason, "in terms of transitional error probabilities (TEP) – the probability of recalling an item other than the one actually following the probe in the sentence", rather than by a more transparent metric, like percent correct.

No feature of this task was able to distinguish syndrome type among the subjects (5 Broca's and 4 Wernicke's): there was no significant group effect, and neither group generated significant interaction statistics with any of the dependent variables. Of particular interest to the functor deficit hypothesis, and to this thesis, is subject performance with respect to word class: when the target was a content word and the probe a functor, subjects produced a mean TEP of 47; when function words were probed for with contentives, the response rates produced a $\bar{x}$ of 75. (No subgroup statistics are supplied.) This means that subjects more reliably recalled content than function words, and the difference proved significant at the impressive level of $\alpha=0.005$. In turn this indicates that functors are less stable psychologically than content words, at any rate with respect to short term memory, for victims of either Wernicke's or Broca's syndrome.

Kolk (1978b)

In a replication of Zurif *et al.*'s (1972) influential test of Broca constituent structure judgements, Kolk (1978b) uncovered a somewhat surprising result: agrammatics treat adjectives much as they treat functors. Kolk drew one class of stimulus sentences from Zurif and Caramazza (1975), and subjected his data to the same hierarchical cluster analysis, but he also included an analogous stimulus set which substituted adjectives for function words, as in 39 - 41.

39. Old sailors tell sad stories.
40. Rich people pay high taxes.
41. Little girls eat sweet cookies.

His subjects were six Broca patients, divided into equal pools labelled "Severe" and "Recovered". The Severe group produced clusters like those of the earlier experiment's Broca subjects: they grouped functors with other functors and content words with other content words for the Zurif-Caramazza stimuli. The Recovered clusters reflected a phrasal constituency strategy – just like the control clusters in the earlier work – for the adopted stimulus sentences, and this pattern held for Kolk's innovations (e.g., *old* was grouped with *sailors*, and *sad* with *stories*). But the Severe group tended to lump adjectives with other adjectives and nouns with other nouns or with the verb. That is, they didn't follow a content vs. function word strategy, but a 'noun + verb vs. other' strategy. This indicates that the Broca subjects aren't necessarily ignoring constituency; on the contrary, they may be paying particular attention to one major constituent, the S.

The implications of Kolk's (1978b) findings for the original studies are obvious. However, the experiment cannot be regarded as any more reliable than the Zurif *et al*. (1972) original, since the subject pool is very small and not very well controlled.

Samuels and Benson (1979)

Similar problems attend Samuels and Benson (1979) which, like Kolk (1978b), also claims to be mining a specifically syntactic deficit in anterior aphasia. The subject pool was not particularly small, at least by aphasiology standards, but it was very heterogenous. There were some differences of educational level, and a very marginal difference in mean age. But the duration of aphasia ranged from four months to almost ten years for the anteriors and only from one to ten months for the posteriors. Anterior production ranged from "single word output with marked dysarthria to fully grammatical but dysprosodic conversational speech" (277), while the posterior group included both Wernicke's and transcortical sensory aphasia. Further, there were twelve anteriors, six posteriors, and five controls, and adjusted means were not used. Consequently, the results are not as useful as they might be.

The tasks were divided into true/false and fill-in-the-blanks tests, with the subject signalling his answer by pointing. The sentences were weighted as indicated by the examples in Figure 2.1. Such sentences

TRUE/FALSE – SEMANTIC
An inch is a unit of wemghh.
TRUE/FALSE – SYNTACTIC
The letter "M" comes béfore"ZZ".
FILL IN THE BLANK – SEMANTIC
homachine that plane is called a ______.
FILL IN THE BLANK – SYNTACTIC
monéckel is worthss ______ than a dime.

Figure 2.1: Sample questions from Samuels and Benson (1979)

were presented auditorily and graphically in groups of sixteen (four semantic and four syntactic auditorily, with the same division for graphic presentation). Accuracy and rapidity of response were measured. Predicted results were achieved: the controls were across the board almost errorless; the posteriors had by far the most errors; and the anteriors fell in the middle, doing almost as well as the controls for semantically weighted stimuli, and almost as poorly as posteriors for syntactically weighted stimuli. As above, the claims of statistical significance for these differences cannot be appraised. There was no difference found in mode of presentation. Response times are reported, but left uninterpreted.

Since all the "syntactic" tasks centred on functors like *after* and *more*, Samuels and Benson (1979) offer some support for a conceptual agrammatism position. But it is very marginal support.

Bradley *et al.* (1980)

Bradley, Garrett, and Zurif (1980), reporting on Bradley's doctoral work (see also Zurif [1982a]), discuss two visual word recognition tasks in which Broca damaged patients were compared to a neurologically sound control group. The first task concerns frequency sensitivity: the more frequent a word is, the more rapidly it is identified in a Yes / No lexical decision; *cat* gets a "Yes" reaction faster than *ape*. The second task concerns left-to-right scan order: nonsense words with an initial substring corresponding to real words are rejected slower in a Yes / No lexical decision than words without real word interference; *houselt* gets a "No" response slower than *touselt*.

Both of these results are well established, the authors claim, and their work only serves to corroborate them. But Bradley also found that these response patterns, for normals, hold only when the real elements are content words. So, in the first task, *the* is not recognized more rapidly than (the less frequent) *or*. In the second task, *theop* is not rejected slower than *speop*. The story is different for the Broca group. As expected, latencies were slower across the board for them, but their response patterns were fundamentally the same for both word classes. They demonstrated frequency sensitivity to functors as well as content words, accepting *the* before *or*; and they demonstrated left-to-right real word interference when the initial substrings corresponded to functors, rejecting *theop* slower than *speop*. Bradley *et al.*'s findings do not directly tap a comprehension problem in Broca's aphasia, but if they hold, they clearly reflect a feature of the syndrome that could substantially affect perception patterns.

However, it must be noted that the results are not uncontroversial. Gordon and Caramazza (1982) report that they were unable to replicate Bradley's findings on frequency sensitivity for normals, as do Segui *et al.* (1982). Gordon and Caramazza, in the following year, also tested fluent aphasics against agrammatics for differential treatment of open and closed class lexical items "spanning a wide frequency range" (1983:337). No significant variance was found here either. On the other hand, Zurif and Grodzinsky (1983:209n1) report on two (unpublished) studies that support Bradley *et al.*'s (1980) contention that there is a qualitative difference between the way normals and Broca patients process lexical items, specifically functors. On a more agnostic note, a recent paper by Bradley and Garrett (1983) advances the same general thesis of their earlier work ("Broca's aphasics are successful in recognizing elements of the closed class, but fail to do so *in the way that normal speakers do*" [156; italics original]), without mentioning either the Gordon-Caramazza papers or Segui *et al.* (1982). Taking the hopefully reasonable assumption that neither camp is dishonest, it appears that this is one of the many areas in aphasiology requiring more extensive investigation.

Swinney *et al.* (1980)

A similar study, following a different paradigm and controlling for stress, reports consonant results. This study – Swinney, Zurif and Cutler (1980) – was discussed above, in the section on aphasic perception of stress, but a significant finding was finessed and now requires consideration. Recall that Swinney *et al.* set up a task requiring eight Broca subjects to respond upon hearing a target word in an aurally presented sentence. The subjects responded more quickly to target words carrying stress than words without stress. This is only half the story. The experiment also included functors as target words, controlled for stress as indicated by the stimulus sentences of 42.

42 a. I think my brother is thé man for this job.
b. I think my bróther is the man for this job.

When this class of stimuli is considered, the results are not as straightforward as suggested in the previous discussion. For the control group, it turns out that the interesting variance in reaction times reported above only holds for function words. Functors were recognized more quickly when stressed, with a whopping Type I probability of less than 0.0001, but no significant differences resulted as a function of content word stress; as might be expected, word class by stress interaction was also highly significant ($p<0.002$). The Broca group, on the other hand, generated significant latency differentials as a product of word class and sentential stress ($p<0.025$), but no notable word class by stress interaction.

So Broca subjects appear distinct from normals on two counts: (1) stress is a salient perceptual cue for them independent of word class, while neurologically sound individuals exploit stress only for function word recognition (at least in some experimental tasks); and (2) they respond differently as a function of word class independent of stress, while normal responses depend upon stress for one word class (functors). Swinney *et al*. (1980) align these results with those of Bradley *et al*. (1980) and use the conjoint to argue that "the special access and retrieval process which underlies ... appropriate use [of closed class words] in normal comprehension has been disrupted" (141) in agrammatics. The questionable reliability of Bradley's results can be suspended here, but it remains unclear exactly how the Swinney *et al*. results support a specific functor disruption in Broca's syndrome.

The principal difficulty of interpretation resides, as in Bradley's work, with the comparison of control and aphasic results. The control group produced no effect for stress on content targets, yet there was a (highly significant) stress effect for function target words. There was also a word class by stress interaction. Further, their reaction times "were significantly faster for open than for closed class materials when they were unstressed [$p<0.007$]" (139). This means, despite the report of a nonsignificant main effect for word class, that the control group evidenced a clear distinction between functors and content words, just like the Broca subjects. The central difference in the results, then, lies with the ability of normals to react quickly to content target words, irrespective of stress. The authors are careful to point out that the inconsequence of stress here is not the artifact of a floor effect, citing evidence from an unpublished masking experiment of Swinney's (139n2). However, a more obvious supposition is that a ceiling effect was in operation. That is, stress becomes extraneous for normal language users when there are a sufficient number of clues already present. Consider the stimulus sentence given in the previous discussion as 9 (and repeated here for convenience).

9. The umpire said a new ball was necessary.

Given that the subject has been told to watch for *ball*, the immediately preceding string (*the umpire said a new*) raises the probability level very substantially that the target word is on its way; and 9 is by no means unrepresentative. On the other hand, function word distribution is not nearly so predictable. Pragmatically and semantically there are many fewer cues; syntactically, they occur at phrase boundaries (usually phrase-initial), where predictability is minimal.

The findings of Swinney *et al*. (1980) can be reduced to the facility of normal language users to exploit pragmatic, semantic, and syntactic context more efficiently than aphasics. This is not a startling result.

Schwartz *et al.* (1980)

As part of a rather ambitious foray into "The Word Order Problem in Agrammatism", Schwartz, Saffran, and Marin (1980) ran two locative preposition experiments with a small subject pool of five "classically agrammatic" (251) patients. This group was somewhat distinguished by heterogeneity: four were CVA victims, one suffered a trauma; output capabilities ranged from entirely holophrastic to occasionally telegrammatic; scores on the Goodglass *et al.* (1975) Syntax Retrieval Test were between 2 and 7. This diversity is reflected in the experimental results.

One experiment involved 48 semantically reversible locative sentences, using the same two geometric shapes. Sentence 43 is an example.

43. The circle is in front of the square.

These sentences were verbally presented in conjunction with three line drawings – one representing the true spatial relations of the sentence; one "reversed role" distractor; and one "lexical" distractor, which represented the relation of a completely distinct preposition (a sample set of drawings is provided). The task proved "extremely difficult for all five subjects" (256): 140 errors resulted from 240 trials, and the best subject had a 42% error rate. Significantly ($p<0.05$), only 12% of the errors were lexical, so the subjects apparently understood the preposition, but were unable to discern which NP it was predicated of, and which belonged to the predicate. For the purposes of this thesis it is particularly important to note that the stimulus sentences were spoken twice, "at a slow conversational pace with equal and heavy stress on the nouns and the locative prepositions" (255). Clearly, this could well have significantly boosted comprehension of the prepositions.

The second prepositional experiment was essentially a replication of the first, but the line drawings included a number of "action scenes" in which "stick figure characterizations of circles and squares [were the] protagonists" (259). Consequently, it also included a number of test sentences on the order of 44.

44. The square is shooting the circle.

The lexical distractor was dropped, and the subjects' responses were constrained to either the correct depiction or its reverse. The goal was to find out if the action (verbal) sentences patterned differently than locative (prepositional) sentences, but the results were not clear. Again subjects had a good deal of difficulty and there was very little between-subject consistency.

One subject is a good representative of the tenor of this paper's overall results:

> in [the last experiment] he appears to have adopted a NP_2-subject strategy in the face of locative sentences but responded indiscriminatly to the order of noun phrases around the transitive verb. However, in [another experiment with inherently animate NPs] ... which utilized the same verbs in active and passive voice, [he] showed a

> tendency (significant at the .03 level) to use and generalize the canonical S-V-O mapping order. In [the first experiment], he manifested no consistent strategy of any kind. (261)

Schwartz *et al*. (1980) use these results to support their position that a specifically syntactic defect is responsible for specific agrammatic word order comprehension problems, but the conclusion is greatly underdetermined. In fact, nothing of any consequence comes from the experiments, except perhaps further confirmation that Broca patients are comprehensionally impaired.[13]

Caplan *et al*. (1981)

In a test clearly fashioned after the Heilman and Scholes (1976) model, Caplan, Matthei, and Gigley (1981) found some conflicting results – part of which support a functor deficit hypothesis, part of which are in conflict with it. They read their subjects thirty randomly ordered sentences, six sets of five, following the paradigm illustrated in 45.

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[13] Caplan (1983) offers a reinterpretation of Schwartz *et al*.'s data which he claims demonstrates "there is no 'word-order problem' in agrammatism" (164), but he falls well short of his goal. First, he is forced to disregard some of the data (that of the subject described in the above quotation). Second, he complains Schwartz *et al*. "do not give actual scores", but observes they are somehow "recoverable" (157), and his analysis uses these reconstructed scores. And third, he proposes a group of order principles the patients might be following in their interpretation of the stimuli that lead to errors on some items. But if they are dependent on novel strategies to analyze the sentences, particularly as those strategies generate incorrect readings, they obviously do not have access to normal speaker knowledge of word order. Certainly Schwartz *et al*. (1980) do not establish a specific word order defect concommitant with agrammatism – their results are far too muddy – but their work is suggestive, and not mitigated in the least by Caplan (1983).

45 a. Can you show Bill walking the dog?
b. Can you show Bill the walking dog?
c. Can you show Bill the walking of the dog?
d. Can you show Bill slowly walking the dog?
e. Can you show Bill the slow walking of the dog?

The task – model manipulation demonstrating the action of the matrix verb complement – was quite difficult by aphasic experimentation standards, but has a venerable history in first language acquisition studies, and Caplan *et al*. report no particular difficulties. Figurines representing the principals (e.g., Bill and the dog) were supplied, and a special 'watching place' was established. Unqualified support for a functor deficit would have been obtained if all subjects (11 Broca's aphasics "of varying severity" [151]) had treated the five sentences as ambiguous or as determined by a simple N-V-N = AGENT-ACTION-PATIENT strategy (i.e., Bever's [1970:298] Strategy B). This did not occur. In fact, as a group, the Broca patients exhibited sensitivity to the crucial functors (*the* and *of*), and to the adverb morpheme (*-ly*). The error rates were of course abnormal, but the only strong group pattern to emerge was the correct deciphering of the stimulus sentences.

Caplan *et al*. requires fairly careful reading, due to an abstract coding system and reader-hostile statistical procedures, but it is clear "that the responses are not totally dictated by admissable grammatical structures" (154-5). Despite the tendency to perform the correct manipulations, there was a contingent of answers that fell into another pattern: the systematic interpretation of all (embedded) sentence types as simple AGT-ACT-PAT clauses. That is, a component of the results support the functor deficit hypothesis. This component was provided by a subgroup of six patients. Unfortunately, very little information is given for this group, but three specific variables – presence of dysarthria, overall severity, and age – were examined as a function of response, and one proved significant: age. Significance may or may not be coincidental here (age seems an unlikely determinant of a functor deficit), but the general results are quite clear on one point. There is no across the board loss of function words in Broca's aphasia. Still, there is sufficient action in the findings that a functor deficit cannot be dismissed out of hand.

Seron and Deloche (1981)

Further, though somewhat marginal, support for a specific functor deficit in Broca's aphasia comes as a byproduct of Seron and Deoloche's (1981) test of Jakobson's well-worn, but still not retired, Regression Hypothesis. They also presented their subjects (36 Broca's, 25 Wernicke's, 9 anomic) with a simple model manipulation task. The stimulus sentences were just as simple – all having the form "put the x {*in*, *on*, or *under*} the y", where x was a movable item and y a fixed referent. Two 'conditions' of x and y were maintained, a pragmatic condition (e.g., x = a coin and y = a piggy bank), and an awkwardly labelled "perceptivomotor contextual bias" condition. For this second condition, x was always a small cube, and y ranged across three abstract objects, each of which exercised a bias toward one of the prepositions (i.e., one had a prominent declivity, which biased for *in*; another had a prominent flat surface, biasing for *on*; the third was raised on legs, leaving a prominent space below and biasing for *under*); all ys were called *object*. Consequently, prepositions could be used both congruently and incongruently, so that the stimulus set patterned as illustrated in Figure 2.1.

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pragmatic bias

congruent:	Put the coin in the piggy bank.
incongruent:	Put the coin under the piggy bank.

perceptivomotor contextual bias

congruent:	Put the cube in the object [with declivity].
incongruent:	Put the cube under the object [with declivity].

Figure 2.2: Stimulus set patterns from Seron and Deloche (1981)

The Broca subjects, across the board, responded more accurately to the congruent requests than the incongruent ones ($p<0.05$ for pragmatic items; $p<0.01$ in perceptivomotor contexts). Wernicke subjects were also more accurate as a function of congruency, but only under the second condition. That is, probable real world spatial relations were only a factor for the Broca patients.

More to the point, the Broca responses were observed to follow "strategies relying on pragmatics" (78), whereas the Wernicke errors were more random. As above, the support here for a loss of function words in anterior aphasia is limited, but the results are consonant with the hypothesis that Broca patients process major lexical items well and compute sentential meaning as a kind of anagram, where Wernicke patients follow a more rudimentary perceptive (visuospatial) program. (The Regression Hypothesis, of course, was unsupported).

Mack (1981)

Following Seron and Deloche – literally, in the same volume of *Brain and Language* – Mack (1981) reported findings which are much less compatible with a functor deficit and which, possibly, are more revealing about the actual problem. He employed a modified Token Test designed to focus on prepositional comprehension, and scored it to evaluate independently syntactic and semantic competence: a "syntactic error consisted of touching or placing a token in the incorrect prepositional relationship" (84); semantic errors involved a confusion of one (or more) of the tokens' attributes (size, colour, and shape). Methodology need not be rehearsed here.

A neurologically sound control group performed almost flawlessly, which arouses little surprise, but the nonfluent aphasics (n = 6) were not far behind, with a mean syntactic score of 7.28 (out of a maximum 8), and a semantic error rate of only 0.77 (in 27 potential confusions). In fact, on two of the three subtests, there was no significant difference between the controls and nonfluents on the syntactic measure. The fluent aphasics (n = 6) were another story. They had an average syntactic score of 2.76 and a semantic error rate of 7.13. Fluent vs. nonfluent *t*-tests proved significant on all measures (with a probability range between 0.001 and 0.05); if there is a peculiar syntactic deficit to be found anywhere in these results, it belongs to the fluents.

Mack's (1981) findings are not the expected ones. In particular, the stellar performance of the nonfluents and the gap separating them from the fluents violate previous findings. Recall that the Token Test is not especially effective for discriminating between Broca and Wernicke patients (Orgrass and Poeck [1962], and Poeck *et al.* [1972]). However, the mystery is not as great as it might seem. Mack (1981) used a modified version of the TT, and only tested for comprehension of advanced structures, as illustrated by 46 - 48.

46. Touch the chip next to the small white circle.
47. Put the blue chip below the small red circle.
48. Put the small white circle farthest from the largest white square.

These requests differ markedly from De Renzi and Vignolo's

(1962) original set. Only two of Mack's (1981) eight prepositions appear anywhere in the original Token Test, and the advanced sections focus primarily on adverbial subordinate clauses. At first blush this indicates that Broca patients are in fact quite good with prepositions, though maybe not so good with adverb phrases. Yet such a conclusion would then violate Seron and Deloche's (1981) results; namely, that Broca victims can exploit pragmatic relationships, but are not very good with prepositions.

However, Seron and Deloche's set of prepositions was smaller and, more significantly, only one of them receives sentential stress in normal assigment: *in* and *on* are always reduced and cliticize to the following noun (determiner or adjective). All of Mack's prepositions are stressed in normal speech (46 - 48 are representative). Further, Mack's prepositions are simply longer, all being at least two syllables, and several being compounds (e.g., *next to* and *farthest from*). As the work reviewed in previous sections demonstrates, stress and duration are crucial factors in Broca victims' comprehension. There are no specific controls to this effect, but considering Seron and Deloche (1981) and Mack (1981) in concert, it appears that reduced prepositions are not processed very well and that stressed (longer, louder) prepositions are processed quite well – something which cannot be smoothly incorporated into a syntactic or functor deficit hypothesis.

Friederici (1981), and Friederici *et al.* (1982)

A somewhat more fundamental tenet of conceptual agrammatism is challenged by the data presented in a series of papers engineered by Angela Friederici. The first two of these papers – Friederici (1981), and Friederici, Schönle, and Garrett (1982) – report on parallel tests of production and perception that employ the same prepositions. The findings are not wholly consistent and are of questionable robustness, but both studies uncover statistically interesting variance in the Broca results. This supports the widespread clinical impression of impaired production concommitant with relatively intact comprehension. But since the motivating force behind conceptual agrammatism (as well as the elegance it lends to a competence analysis), is founded on a hypothetical higher-order deficit uniformly generating symptoms in production and perception, this finding is less than consonant with the theory.

Friederici (1981) reports on two distinct experiments, each organized around a set of line drawings, and each requiring both production and perception by the subjects (6 Broca's, 6 Wernicke's). The first experiment used 11 drawings which represented locative relationships holding "between simple objects like 'ball' and 'box'" (193). For the production task, the patients were asked to provide the "little words" depicted by the items of the drawing. To avoid ambiguity (since, e.g., "the ball is *in front of* the box" is equivalent to "the box is *behind* the ball"), they were directed to one object; for instance, "to describe 'where the ball is'". The comprehension component called for selection of the appropriate preposition, from a visual array of four, corresponding to a presented drawing. The second experiment's stimulus set substituted "real life situations" for the context free depiction of objects. Friederici's examples are given here as 49 -51.

49. The cat is lying *under* the chair.
50. The girl is standing *behind* the tree.
51. The smoke is coming *out of* the chimney.

The production task here was sentence completion. The drawings were presented coincidentally with a visual and an aural token of an incomplete sentence, a sentence like 49 - 51, without the italicized sections, and the subject was asked to fill in the absent word(s). Precisely the same format was followed for the comprehension task, except an array of four prepositions was also supplied, and the patients were required to choose the appropriate word with which to complete the sentence.

There are some obvious problems with these paradigms. For instance, it might be objected that the comprehension subtests were variants of the production task which bypassed the need for lexical search. It might also be objected that instructions to provide "little words" are too vague for victims of aphasia. These are legitimate concerns. There is a problem of statistical analysis as well: Friederici (1981) supplies three pages of figures and graphs, but they all appear to be the product of a standard computer software package, with very little in the way of thoughtful investigation. For example, a four-way ANOVA design provides information on 16 potential sources of variance, so it is not a tool to be used lightly or without some effort at interpretation; Friederici reports on seven effects, and drops the numbers as if they were radioactive. Neither, for all the figures floating around, is there clear indication of how the two groups fared on the tasks with respect to one another. Production tasks proved to differ very reliably from perception tasks ($p<0.0005$), and the task by group interaction also proved significant ($p<0.01$). But most of the variance appears to be a product of the Broca subjects' performance: their production mean was 5.7 (%-correct) and the perception mean, 62.1; the Wernicke subject means were 54.95 and 87.15 respectively. It would appear the Broca pool accounted for a larger measure of the variance than the Wernicke pool, yet this is statistically unexamined and receives no commentary in the text. Still, while the data are compromised, experimentally and analytically, they cannot be rejected out of hand. It remains interesting and runs counter to a functor deficit hypothesis. The existence of a group by task interaction, particularly one that appears to be largely a Broca product, is not consistent with a single underlying deficit manifest in both comprehension and production. Notice also, that the sample sentences include multisyllabic or compound prepositions, which seem to be better processed by Broca patients than shorter ones.

Some of these problems are cleared up in the study reported by Friederici, Schönle, and Garrett (1982). It used the same stimulus materials as the second experiment discussed above, and possibly some of the same subjects (the age means are very similar), but another subtest was added. The production task remained sentence completion, and one comprehension task was prepositional selection, both following the methodology just outlined. The addition was a sentence selection test: the line drawings were presented along with four random sentential choices; one sentence was correct; one contained an incorrect preposition ("the cat is lying *on* the chair"); one contained an incorrect prepositional object ("the cat is lying under the *table*"); and the fourth involved some violation of a linear order constraint ("the cat is lying *the under* chair"). This subtest is a more obvious measure of comprehension, calling for less in the way of metalinguistic analysis than a fill-in-the-gap preposition task; it is also sufficiently divorced from the production part of the experiment. Statistical analysis reveals more thought in this paper as well. A simple 2-way ANOVA was carried out, crossing subject group by task, and a main effect for task was uncovered ($p<0.01$). Planned *t*-tests were then executed, and the results support the impression that Broca subjects account for most of the variance in a group by task interaction. No significant differences were found in the

Wernicke data, but production against prepositional selection was again reliably differentiated (α=0.01) for the Broca victims. On the other hand, testing production against sentence completion did not reach significance until α=0.2. That is, when the design is cleaned up and the analysis more principled, the threat to conceptual agrammatism appears to diminish. But even a significance probability just under 0.2 requires some explanation; it signals with a confidence level of 80% that the perception trial scores were higher than the production scores for the same set of prepositions.

It should be noted that Friederici, in both papers, subscribes to "the view that Broca's aphasics' deficit is primarily an inability to use their syntactic knowledge source ... to access a specific fixed (closed) class of items" (Friederici [1981:198]), and she would be loathe to draw the conclusion that her data undermine a syntactic incompetence analysis. In fact, Friederici *et al*. (1982:532) claim "to have provided some evidence ... that agrammatics' comprehension performance parallels their production performance". This is far from the case.

Friederici (1982)

In a much better study, with clearly defined questions and a more systematic design, Friederici (1982) produced the same results – perception and production tasks involving the same prepositions generated significant variance – and this time the figures are much more compelling. The experiment is similar to her earlier work just reviewed, but it is distinguished by two important features: a fair measure of semantic control; and the fact that it is one of the very few foreign language aphasia studies published for an English audience. It also produced highly convincing numbers, working from a larger subject pool, and employing more appropriate statistical procedures.

German allows for a particularly nice 2-way test of prepositional content vs. prepositional function. There is a class of prepositions that bear a certain amount of semantic information, as in 52.

52. Peter steht *auf* dem Stuhl.
(Peter stands *on* the chair.)

Another class of prepositions, comprised of phonologically and etymologically identical elements, has a distribution dependent on certain verbs. That is, they are structural requirements and have no meaning distinct from the verb; in generative terms, some verbs are strictly subcategorized for some prepositions. Sentences 53 and 54 are examples.

53. Peter hofft *auf* den Sommer.
(Peter hopes *for* the summer.)

54. Peter achtet *auf* das Feuer.
(Peter watches Ø the fire.)

These two classes have precisely the same combinatory properties – they head either PPs or Ss (i.e., they topicalize) – and are distinct from particles (which cannot head Ss) that also share historical and phonological identity. The crucial difference, then, is semantic.

Friederici (1982) quite deftly exploited this distinction, in what is by far the cleverest and most valuable application of her production / perception format. Her production task was once more sentence completion, although this time "a little story describing the circumstances" (253) was used to establish context, as opposed to the line drawings of her earlier experiments. The choice was prompted by a desire to keep the test "purely linguistic", but it may have confounded the experiment somewhat by placing more demand on comprehension and, perhaps, lessening the productive element of the task. However, there is always some tradeoff in linguistic vs. nonlinguistic stimulus items and, in any event, whatever complications might have been introduced came out in the wash; interesting production against perception variance not only still resulted, it is the cleanest result in any of her three papers. The comprehension task called for grammaticality judgements of forty-six sentences. Fourteen contained 'semantic' prepositions (52) and fourteen were 'syntactic' (53 - 54). The remaining twenty-eight were anomalous, with half violating semantic restrictions (e.g., 55) and half violating syntactic co-occurrence restrictions (56).

55. *Peter steht *durch* dem Stuhl.
(Peter stands *through* the chair.)

56. *Peter hofft *über* den Sommer.
(Peter hopes *over* the summer.)

The subjects (12 Broca's, 12 Wernicke's) were asked simply to judge "whether the sentence was correct or not" (253).

The perception data were corrected for a chance level (50%), and all numbers were run through a nested (subjects) ANOVA. The main effect for task type again proved very reliable (an error probability of 0 is given – meaning, presumably, that it calculates beyond 4 decimal places), and a good measure of the variance again stemmed from the Broca patients' performance, for whom task type was significant at a $p<0.0003$. Such definitive significance statistics add well-needed robustness to the findings of Friederici (1981) and Friederici *et al.* (1982), making them all the more problematic for conceptual agrammatism.

Friederici's (1982) explorations of the semantic dimension were also rewarded, to some degree. She found a curious dichotomy in the Broca results: the agrammatic subjects were much better at producing the semantic prepositions than the syntactic ones ($\bar{x}=69.64$ vs. $\bar{x}=36.31$; significant with $p=0$); but there is no evidence that meaningfulness played a similar role in the comprehension task ($\bar{x}=92.28$ vs. $\bar{x}=91.08$; nonsignificant). The Wernicke patients demonstrated no such dichotomy, producing no statistically interesting interaction between task and preposition type. It is not clear how to interpret these results. On one level, they serve to reinforce the impression that Broca production and Broca perception are fundamentally different beasts. But there are alternate accounts. Most notably, it may be that the semantic distincion outlined above dissolves when the task at hand is grammatical evaluation. Sentence 56 involves co-occurrence restrictions, while 55 does not, but the concept of *hoping over the summer*, at least without elaborate contextual justification, does not seem any more sensible than *standing through a chair*. Even if the semantic dimension does not dissolve in such tasks, it may be obscured by a ceiling effect. A second possibility – less likely but dearer to the heart of this thesis – is that a ceiling effect was in operation as a function of the properties investigated in previous sections; German prepositions are not reduced, as their English counterparts are, and the widespread features of diphthong and umlaut make for longer average syllable durations. And, of course, the ceiling could be supported by both semantic and sonic factors. However, this has only incidental bearing on the implications Friederici's work has for the functor deficit hypothesis, a hypothesis more and more difficult to sustain as the data come in.

Kudo *et al.* (1982)

Another rare foreign language study – Kudo, Kashiwagi, and Segawa's (1982) investigation of Japanese Broca patients' functor perceptions – also provides data incompatible with the function word deficit hypothesis, and Japanese has some properties particularly valuable for testing that hypothesis. It is agglutinative, at least with respect to grammatical relations, and it codes verb arguments with post-positioned particles. For instance, 57a and 57b have the same propositional meaning (a boy [*shōnen*] hit [*naguru*] a girl [*shōjo*]); subject is signalled by the particle *ga*, while object is signalled by *o*.

57 a. Shōnen ga shōjo o naguru.
b. Shōjo o shōnen naguru.

In the passive form the oblique (BY-)object is coded with the particle *ni*, as in 57c and 57d (an intransitive marker also inflects the verb).

57 c. Shōjo ga shōnen ni nagurareru.
d. Shōnen ni shōjo ga nagurareru.

In short, functors (case particles) are the essential cues of grammatical relations in Japanese, as essential as word order in English, and this makes for rather fertile ground in which to dig for data relevant to conceptual agrammatism.

Kudo *et al*. (1982) see the situation this way as well; unfortunately, the fertility of Japanese here may have been a little intoxicating, and they tackle the subject with a complex assortment of sentences, 64 in all. Four distinct factors were brought into operation, each crossed with the others: (1) semantic reversibility of subject and object, (2) voice, (3) word order, and (4) truth value. Reversible sentences in two word orders (SOV [basic] and OSV) and two voices (active and passive) are presented above. The sentences of 58 are the nonreversible corollaries.

58 a. Kuruma ga kodomo o haneru.
b. Kodomo o kuruma ga haneru.
(A car runs over a child)

c. Kodomo ga kuruma ni hanerareru.
d. Kuruma ni kodomo ga hanerareru.
(A child is run over by a car)

All sentences (16 groups of 4 following the above paradigms) were presented randomly to the subjects (20 Broca's, 10 Wernicke's, 10 amnesics) in conjunction with a line drawing that depicted figures and an action based on the content words of the sentence (e.g., *a boy hitting a girl*). The task, then, was sentence verification, and truth value was determined as a function of the sentence and picture combinations (57a would be true with respect to the example just given). Further, there were two conditions of 'falseness' in the test, illustrated by 59a and 59b (with respect to the same example).

59 a. Shōjo ga shōnen o naguru.
   b. Shōnen ga shōjo ni naguru.

That is, one sentence (59a) represented an inaccurate subject-object relation, and the other represented a syntactic violation (a subject particle and an oblique particle teamed with an active verb).

An ideally constructed experiment, both in terms of focus and statistical dependability, is one that produces figures testable with a tool like the *t*-test. Kudo *et al*. (1982) employed, and required, two distinct five-way analyses of variance: ANOVA1 took Aphasia Subtype, Reversibility, Word Order, Voice, and Truth Value (T vs. F) as its factors; ANOVA2 followed essentially the same design, but analyzed the two types of falseness as the fifth factor (F_1 vs. F_2). The results include some significant numbers, which is to be expected, but interpretation is not at all straightforward. Aside from the fact that multilevel ANOVAs invariably hook some significant F-statistics (the more tests conducted, or levels of analysis, the more chance of committing type I errors, even at quite low α-levels), there are serious difficulties with the meaning of significant Fs here. For instance, sentences of type 59b involve only one inappropriate case marker (*ni*), but 59a involves two inappropriate particles (*ga* and *o*), and for the former the line drawings are irrelevant, while it is the line drawings that identify the inappropriate particles in the latter. Further (and of more concern to this thesis), Japanese word order is free in principle only. In practice it is SOV, and departures from that order are marked constructions, usually attended by increased particle stress; in terms of the examples given above, the functors of 57a and 57c are systematically reduced, while those of 57b and 57d are longer, louder, and higher in pitch.

Still, there are two aspects of the general pattern in Kudo *et al*.'s results that can be reported with some confidence. First, Truth proved a massively significant factor, with an F of 204.37, and "the performance difference between true and false sentences in the Broca's aphasics was far more salient than that in the Wernicke's and the amnesic aphasics" (647). (Aphasia Subtype [$p<0.01$] and the Subtype x Truth interaction [$p<0.01$] were both significant.) So the difficulties observed in English Broca patients with functors are also present in Japanese patients. Second, this deficit is context-sensitive: Reversibility generated interesting variance ($p<0.05$), as did the Truth x Voice (p<0.01) and Voice x False Type (p<0.05) interactions. That is, detectability of inappropriate functors varied with "the linguistic environment in which they [were] embedded" (648). The functor difficulties of Japanese aphasia patients, like those of English patients, are not independent of other linguistic, or extra-linguistic, factors.

Linebarger *et al.* (1983a)

The title of Linebarger, Schwartz, and Saffran's (1983a) paper reveals tenor and aim immediately: "Sensitivity to Grammatical Structure in So-Called Agrammatic Aphasics". It is a clear broadside at conceptual agrammatism – in particular, at its syntactic manifestations. To this end they designed a test battery calling for grammatical evaluation of 451 sentences, 221 of which were ill-formed, representing violations of 10 classes of structures. As always in such things, there was a trade-off between thoroughness and generality; detail of experimental design took precedence for Linebarger *et al.* over subject representativeness, and their sample consisted only of "four adult females, victims of organic brain damage ... aphasics of the Broca type" (367). Consequently, the claim of "providing strong counter-evidence" (362) to a syntactic analysis of the syndrome is a little over-zealous. Nonetheless, their results do run in opposition to syntactic accounts, and they include some specific implications for a functor deficit hypothesis.

Several of the stimulus sentences hinge on function word distributions, valid and invalid. For instance, the difference between the following well-formed sentence (60a) and its ill-formed mate (60b)is the presence of *to*.

60 a. The policeman was talking to a woman.
b. *The policeman was talking a woman.

Likewise, the difference between the valid 61a and invalid 61b depends on the appropriateness of a function word.

61 a. I hope you will go to the store now.
b. *I hope you to go to the store now.

And similar patterns are responsible for the syntactic violations of most other sentence types in the experiment. Obviously, there is more to these ill-formed sentences than the presence, absence, or appropriateness of the function words. For instance, in 60 the knowledge that the verb is intransitive suffices to reject b, since intransitive verbs cannot be immediately followed by NPs. Knowledge that *hope* requires a modal, or perhaps that it requires an irreal proposition, is sufficient to reject 61b. But in both cases some access to functors is required, even if only to category membership (e.g., modal vs. preposition). If Broca patients process sentences as content word telegrams, irrespective of whether those words come labelled *intransitive* or *irreal*, then the two sentences of 60 and the two of 61 would be identical to them. The results show this not to be the case.

Linebarger *et al*.'s findings align with those of Friederici (1982): Broca victims are not particularly impaired in grammaticality judgement, even when judgement hinges on function words. For only two out of the 10 classes of violation did mean percent correct identification of ill-formed sentences drop below 94 (both those cases being still above chance despite a substatial outlier mean of 0.025 for the two structures).

Kolk and Van Grunsven (1984)

A more specific, and less polemical, assault on conceptual agrammatism comes from Kolk and Van Grunsven's (1984) replication of Zurif *et al.* (1972). They asked their subjects (4 agrammatic, 2 'non-agrammatic') to choose the two words from a triadic array that "go best together in this sentence" after a sentence containing those words was read aloud, and while the sentence was on display. There were 18 sentences, evenly divided into the three types represented by 62-64 (the examples are Kolk and Van Grunsven's, but it is not clear if they are direct translations of the originals, given in Dutch, or merely illustrative):

62 The horse kicked my son.
(simple active: Det-N-V-Det/Prep-N)

63 The car is washed.
(simple passive: Det-N-Aux-V)

64 Rich people buy expensive cars.
(simple adjectivals: Adj-N-V-Adj-N)

However, pretest subject preparation was more extensive here than in either Zurif *et al.* (1972) or Kolk's (1978b) replication. First, all subjects were screened for their

their ability to understand the nature of a triadic comparison. They were presented with three content words, two of which had strong semantic ties (e.g., *son*, *wood*, *parents*), and they very consistently chose that pair. Each subject was also given several trials exactly parallel to those of the central test, and

> after he had made his choice for the third triad it was stated that this choice was, of course, perfectly acceptable but that other people could have made a different one. Then the other two possibilities were pointed out to him. It was stipulated that the choice was a personal one, that it was not a matter of "good" or "bad" and that words that were adjacent in the sentence need not be paired (34).

In addition, during the test proper the subjects were required to demonstrate the two alternative groupings after they had selected from each first and sixth triad.

These elaborate precautions that the subjects understood the task resulted in radically different clusters from those of the two earlier studies. The non-agrammatic subjects produced cluster graphs of the pattern claimed in the earlier studies to typify agrammatics. While this finding is not at odds with the earlier studies – it couldn't be, since both of them included only agrammatic aphasics – it does seriously compromise Zurif *et al.*'s claim of "isolating the syntactic impairment" (1972:415) of agrammatism. But the agrammatic clusters are in direct conflict with the earlier runs of the experiment: they take the 'normal' pattern of conjoining words according to phrasal constituency, pairing, e.g., *the* with *horse* and *my* with *son*. Kolk and Van Grunsven offer some interesting speculation about the discrepancy between their data and the previous findings; in particular, that the patients who group content words with content words and functors with functors do so because they are people who know they have difficulty understanding language meaning, know the testers are language therapists, and *expect* to be tested on meaning. But the two important points for this thesis are (1) non-normal judgements (i.e., grouping by some metric other than phrase structure) on this task do not necessarily reflect a lack of structural knowledge, and (2) arguments for conceptual agrammatism depending on the Zurif *et al*. (1972) results are severely weakened.

Smith and Mimica (1984)

Despite this mounting evidence and its attendant arguments, conceptual agrammatism does not appear to be in any immediate danger of extinction. A good example of its healthy constitution is the recent paper of Smith and Mimica (1984). They report on a study of case-marking sensitivity by agrammatics in an inflected language, and they interpret their results in support of an accessing hypothesis which claims that Broca patients have a specific problem accessing syntactic (case) cues.

Their subjects were 10 Yugoslav Broca victims, native speakers of Serbo-Croatian, and 10 neurologically intact Serbo-Croatian Yugoslavs "matched approximately [with the aphasics] for age and educational background" (280). The stimulus items were three-word Serbo-Croatian sentences constructed from four pools of concrete nouns and a verb pool. The noun pools were established according to two binary metrics, animacy and overt case marking. That is, one pool consisted of nouns that were both animate and took overt markers, one pool was animate but did not inflect, one was animate and inflected, another was inanimate but did not inflect, and the fourth was inanimate and inflected. The sentences were generated at random, so that each patient and matched normal had a unique set, but the sets conformed to the following constraints: there were three inflection types, nominative-accusative, accusative-nominative, and ambiguous (where neither noun was inflected; there were two word sequences, N-V-N and V-N-N (word order is a secondary cue of grammatical relations); and there were 3 animacy conditions, animate-inanimate, inanimate-animate, and animate-animate. These factors combine to make 18 sentence types (3 x 2 x 3), and each type was represented by two tokens, for a total of 36 sentences per set. The sentences were taped "in a slow, even voice, and each sentence was repeated twice" (281), and the subjects demonstrated comprehension by model manipulation, a task they "had little trouble understanding and performing" (281), despite the reported hemiplegia. No feedback was provided during testing.

The diversity of variables required three ANOVAs: one within each subject group, and one treating group as a between-subjects factor and the three remaining factors (case, word sequence, and animacy) as repeated measures. There is little need to report on the within normal results here, other than to note that they "were quite similar to those reported for a larger sample of Serbo-Croatian university students" (282). Nor are the within Broca findings of particular moment; in most respects they paralleled the normal group, at a lower level of performance. But the third analysis is more revealing. It found a significant case x group interaction ($p<0.001$) which shows the Broca patients to be considerably less sensitive to case marking. There was also a significant animacy x group interaction ($p<0.009$) which shows the Broca's to rely more heavily on semantic strategies for interpretation, even when those strategies conflict with the case inflections. Further, a *post hoc* comparison of the two groups showed that a convergence of all grammatical relation cues (word order, animacy and overt inflection) resulted in insignificant comprehension differences; the Broca patients had a success rate of 90% with such items. Clearly, these results are in consonance with the major findings of this section: agrammatics have difficulty comprehending 'purely grammatical' information, but the more redundantly that information is coded, the better they do.

As Smith and Mimica (1984) point out, this differential loss — when the information is available in some contexts but unavailable in others — is difficult to reconcile with a uniform loss theory. Grammatical formatives are not lost across the board. The authors do not explore the dimensions of the loss to any depth, suggesting only the uninterpretted data reformulation that the problem involves an inability to reliably access cues of grammatical relation, but a strong possibility is that a contributing factor is acoustic. The Broca subjects were least sensitive to case marking, and case is coded in Serbo-Croatian by unstressed suffixes.

Summary

There is a number of individual problems with the work reviewed in this section — problems of design, of analysis, and of interpretation — but two relatively clear results are extractable. First, Broca's aphasics do not detect functor meaning as reliably as nonaphasics. They have comprehension difficulties, and those difficulties implicate function words. Second, the difficulties do not implicate function words *qua* function words. There is no uniform, class-dependent loss. There are at least three variables — pragmatic, semantic, and acoustic — that condition functor comprehension in Broca's aphasia. Meaning and sentential stress effect a saliency in functors they do not normally possess, and Broca victims respond more accurately as a function of increased salience. (Pragmatic variables most likely work in a precisely reverse manner, by making the functors irrelevant.)

2.2 Theoretical

Theoretical speculation as to the nature of 'pure case' Broca's aphasia has for the most part been elided in the discussions above, and in general theory does not play a major role in this thesis. The principal reason for this apparent omission is that recent aphasia theories are largely proposals of competence dysfunction, and competence – as a psychological reality – is so poorly understood that cognitive accounts dependent on it must be relegated to a scientific anteroom, alongside speculations on the nature of extraterrestrial life, to await relevant breakthroughs in data. Competence theories in nonpathological linguistics, from the Standard Theory to Generalized Phrase Structure Grammar, have essentially been developed and modified according to criteria like consistency, completeness, and parsimony. These are all criteria that responsible pathological accounts also have to meet, eventually. But the most pressing concerns at present do not involve formal elegance. They involve direct empirical research and hypothesis testing. Competence models, intact or with posited malfunctions, are simply not testable in any reliable way.

Nonetheless, two competence theories require some attention here. The first, Zurif's proposal of syntactic incompetence, requires attention because much of the work reviewed above has been taken, by Zurif and by others, as concrete evidence in its support. The second, Kean's theory of phonological incompetence, requires attention because the experiment reported in the following chapter may, superficially, appear to test its claims.

2.2.1 Zurif's Syntactic Proposal

Edgar Zurif's account of Broca's syndrome, advanced in a number of papers by a number of scholars, is of course not strictly Zurif's. As above, it first shows up in Goodglass (1968) as *conceptual agrammatism*, and it has several manifestations that Zurif may not endorse. But he is by far the most enthusiastic and prolific proselytizer, and his research is central to both its formulation and current acceptance. The general character of the account is syntactic: Broca victims have production and perception difficulties that run in parallel because they are forced to traffic in major lexical items by an "inability to satisfy some minimum requirements of a syntactic processor" (Zurif and Blumstein [1978:245]). The exact nature of this inability has not been fixed. In early work it was considered to be semantic (Zurif *et al*. [1972]). Later it was held to implicate lexical retrieval processes (Bradley *et al*. [1980]). More recently it has gravitated toward Kean's "sound-syntax interface" (Zurif [1982b]). (All three suggestions are treated as fully compatible in his work.) The theory has also moved from an explicit competence proposal to one cloaked in the language of performance. But the general character – syntactic incompetence keyed to function words – has been remarkably stable; for example, the 'performance model' Zurif (1982b) briefly entertains employs the "grammar as a component" (206), and the discussion centres on that component.

However, it is important to notice that Zurif has never adhered as rigidly to a functor deficit as previous discussions might suggest. His general claim is that Broca victims comprehend by sampling major lexical items, but aside from popular articles in magazines like *American Scientist*, he has never implied that an across-the-board loss of function words attends Broca's syndrome. He could not support such a claim; data he has been instrumental in collecting argue against it. Recall, for instance, that the early cluster analysis work failed to discriminate between agrammatics and neurologically sound controls by their treatment of the object NP in sentence 17.

17. Where are my shoes?

The stongest cluster for both groups was the conjunct of *my* and *shoes*, forming a recognizable NP. Similar results followed from a later run of the same design (Zurif and Caramazza [1976]), which used an expanded stimulus set, illustrated by 65 and 66.

65. My dog chased his cat.
66. Gifts were given by John.

The cluster graphs for sentences of type 65, somewhat surprisingly, upheld the general results of Zurif *et* *al*. (1972): the control graphs resembled phrase markers, indicating a constituent-governed grouping; the agrammatic graphs followed an apparent contentive-with-contentive, functor-with-functor strategy. Possessives did not affect the response pattern in the second run, and 65 was treated by both groups much like 15 of the original study (*The dog chased a cat*). But in sentences like 66, both subject pools consistently judged *by* 'to go best with' *John*, maintaining the integrity of the PP. In a more recent study, a replication of the Goodenough *et al*. (1977) Token Test paradigm which gave "the patients unlimited time", Zurif and his coworkers found that all the subjects "recognized when an article [was] ... used inappropriately" (Zurif [1982b:205]).

In short, Zurif's research has uncovered a number of situations in which Broca victims evidence "gradations of sensitivity to functors" (Zurif and Caramazza [1976:270]). The two cluster analyses show relatedness to vary as a product of individual lexical item, though sentence type or availability of similar function words (i.e., articles, possessives, and prepositions) may well have been crucial factors. The Goodenough *et al*. replication shows detection of inappropriate article use to improve substantially with increased processing time. The variables were different in both cases, but each patently evinces differential treatment of function words. This differential treatment is also the principal finding of this chapter's survey of function word experiments, of course, and it demonstrates quite conclusively that class membership is not a reliable predictor of agrammatic performance on metalinguistic (or comprehension) tasks; in particular, Broca victims do not respond simply as a function of the contentive vs. functor dichotomy.

This finding automatically vitiates certain peculiarities of Zurif's proposal. For instance, impairment of a specific function word retrieval system is ruled out if some functors are reliably retrieved while others are not. Nor does the semantic argument appear to hold, since *by* in 66 is a mere oblique object marker, with considerably less 'weight' than *my*, or even *the*. But differential treatment has more damaging implications for the general claim that syntactic dysfunction is the heart of agrammatic comprehension difficulties. There is just no reason to suppose that a syntactic component requires access to lexical information beyond category (and subcategory) membership, and categorically *my* is indistinct from *the*; *on* is indistinct from *next to*. They are distinct semantically and acoustically, but not syntactically.

The problem, then, is not that Zurif's proposal cannot accommodate differential treatment of functors; in fact, quite the opposite. The admission of gradations of sensitivity has two undesirable consequents for the proposal: (1) it weakens it to the point where empirical testing is no longer feasible; and (2) it starts the proposal on a slippery slope away from syntax. The first is not desirable because an untestable hypothesis is scientifically vacuous. The second is not desirable because an argument on a slippery slope tolerates both a proposition and its converse: 'syntax is responsible for Broca comprehension difficulties' and 'syntax is not responsible for Broca comprehension difficulties'.

Moreover, a very specific and extensive study which attempts to uncover a syntactic deficiency in Broca's syndrome has produced results that indicate "syntactic competence is not inaccessible either in Wernicke's or in Broca's aphasia" (Lonzi and Zanobio [1983:177]). The four year, 133-subject (53 Broca's, 45 Wernicke's, 35 normal controls) experiment tested for syntactic abilities in four distinct areas: general complexity, infinitival complements with null subjects, deictic forms, and structural paraphrases. It found that the two aphasic groups were virtually indistinguishable from one another on all their tests, and though obviously impaired, they performed on parallel with the normals.[14]

If Zurif's proposal were a straightforward claim of function word loss, it would implicate syntax; more significantly, it would be empirically falsified by, for instance, Mack (1981) or much of Zurif's own research. But as soon as selective loss of function words, or graded sensitivity, is introduced, syntax is extraneous. The interesting variable then becomes whatever the basis of selection is, and there is good reason to believe that a central variable is acoustic.

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[14] However, they found one subtest which provided a distinction between the two aphasic pools: the Broca subjects had more difficulty with the interpretation of a genitive PP, most often judging it to be a simple object NP (Lonzi and Zanobio [1983:177]). That is, they showed less sensitivity to a function word (*di*).

2.2.2 Kean's Phonological Proposal

There is good reason to believe the variable, or a central variable, in the differential treatment of functors by Broca victims is stress-related. The evidence reviewed in this chapter is certainly consonant with such a suggestion, and such a suggestion is the primary thrust of this thesis. Since stress is the province of phonology in linguistics, the evidence is also consonant with a phonological account of Broca's syndrome, which is the primary thrust of a series of papers by Mary-Louise Kean (1977, 1978, 1980a, 1980b, 1981, 1982). And a change of focus from syntax to phonology is in principle an attractive one:

> The likelihood that this endeavour ['biological psychology'] will go far with syntax in the near future is low, because we still know very little about the principles of motor control that might underlie syntactic capacity – that is why [the] current study of syntax is, from a biological point of view, descriptive rather than explanatory. But the prospects are better for phonology, because phonology is necessarily couched in terms that invite us to reflect on the perceptual and motor capacities that support it. (Studdert-Kennedy [1982:329])

However, Kean's position and the position of this thesis are emphatically *not* in consonance. Her theory is competence-based, vaguely circumscibed, and founded on several notions motivated solely by the theory; nor, in Studdert-Kennedy's terms, does it pause much to reflect on perceptual or motor capacities. But its compatibility with the data and its superficial resemblance to the argument of this thesis, demand it receive more than summary dismissal.

As the number of publications indicates, Kean's theory has undergone some modification, but the alterations are minor — principally terminological — and her position is still best characterized by the earliest paper:

> A Broca's aphasic tends to reduce the structure of a sentence to the minimal string of elements which can be lexically construed as phonological words in his language. (1977:25)

The account is admirably succinct, but it is also patently underspecific: what is *lexical construal* and what are *phonological words*? Neither is adequately defined. The former, she admits, "is not a phonological concept", but claims it "is part of the normal language system" (26), not merely an *ad hoc* postulate.

As a demonstration that construal is an activity of undamaged language users, Kean appeals to speech error data. She considers several errors on the order of 67.

67 My *frozers* are *shoulden*.
(for *shoulders are frozen*)

67 is a possible speech error (she implies her examples are attested) because *shoulders*, which has the lexical structure

given as 68a, can be misanalyzed as having structure 68b.

68 a [#[#shoulder#]s#]
   b [#[#[#should#]er#]s#]

This "construal of *er* in *shoulders* as a word-boundary suffix is possible because there is an occurring suffix *-er* (as in *dealers*) and *-er* does not affect stress" (27). On the other hand, 69a is not a possible speech error, since 69b is not a possible construal of *sing*.

69 a. *they *choring sals*.
      (for *sing chorals*)
   b. *[#[#s#]ing#]

Although *-ing* occurs as both a derivational and inflectional suffix elsewhere (e.g., *jumping*), it is not possible to construe a single consonant (namely, *s*) as a phonological word. *Lexical construal*, then, is a process solely concerned with the recognition of phonological words, which reveals its role in Kean's principle as (1) circular, and (2) obfuscating.

The key term is clearly *phonological word*, and *construal* receives very little mention after the first paper. Kean (1977) provides two properties that define her central concept. First, it is a crucial player in the game of prosody (24). Second, it is delimited formally as any string, X, which occurs in the environment, [#______#], such that X contains no # (24). The first property is general and informative, and it demonstrates admirable goodness of fit to Broca's aphasia data; affixes rarely affect lexical stress in English, and functors rarely affect sentential stress. Both members of the following two pairs have the same stress assignments:

70 a. móther
   b. mótherhood
71 a. ^{2}Boys ^{3}climb ^{1}trees.
   b. The ^{2}boys ^{3}climb up the ^{1}trees.

The second property – the formal environs of *mother*, *boy*, etc. – is, of course, theory dependent. However, *phonological theory* is never given much substance in her work.[15] The only specific given of the phonological system she follows is "that of word-boundary assignment: assign a # to the left and right of every lexical item (N, A, V, P, Adv)" (1981:190). The machinery of this assignment is not specified, but her authority is Chomsky and Halle (1968) – and those authors include a definition which may well have been the prototype of property (1):

> Suppose we have a string ...Y... = ...Z[#W#]V, where Z[# and #]V are termini..., and Y contains no other termini. Then [#W#] is a word (367).

But SPE, just prior to this definition, also includes a cautionary note apposite to Kean's work: "it is important to bear in mind, however, that a word, in the phonologically relevant sense, is not simply determined as a string bounded by occurrences of #" (367). In specific, Chomsky and Halle explain that for generative phonology such strings as 72a and 72b, which they analyze (with Kean) as 73a and 73b, meet

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[15] Even reports in the literature sympathetic to Kean's work feel compelled to notice this sketchiness. For instance, after noting Kean's persuasiveness in arguing "that agrammatic behaviour in Broca's aphasics is determined by a unitary impairment", Lonzi and Zanobio (1983:188) note parenthetically that impairment "remains to be described in its mechanisms".

"the definition 'word'" (367).

72 a *differing*
b *metalanguage*
73 a [#[#differ#]ing#]
b [#meta[#language#]#]

So there is no one-to-one correspondence between the notions of SPE and Kean, despite the notational parallels. Klosek (1979:65) notes this discrepancy as well, and Kean responds by drafting another distinction, holding between *word-level word* and *phonological word* (1979:77n10, 1980b:256). This inovation does not alter the fact that Kean's term has no predecessor in the principal document of the phonological theory which apparently underwrites her analysis of Broca's syndrome.

More problematic for her account, however, is the existence of powerful "readjustment rules to 'erase' the word boundaries" (Kean (1981:190)). Chomsky and Halle use this mechanism solely to account for intonation contours not predicted by their regular sentential stress devices (1968:371-2); Kean follows suit, using the rules to mop up data her theory won't otherwise handle (1979:77, 1980b:252, 1981:203). But nowhere, in either SPE or Kean's writings, is a readjustment rule given. Nowhere is there a suggestion as to what one might look like if it were given, when it might be triggered, how it might be constrained, or what purposes additional to an *ad hoc* clean up it might serve. For Chomsky and Halle this is not really a problem: their discussion is brief and they admit the rules are thorny because "it is very difficult to separate the study of these processes from the study of the theory of performance in any principled way" (1968:371). But Kean, who is only interested in competence, simply implies the rules are a well-entrenched feature of a universal phonological theory.

The difficulty here is more than a lack of detail: even if it were possible to assume Kean's phonological framework was essentially identical to SPE (and to assume that framework has no difficulties of its own), her invocation of readjustment rules makes her account of Broca's syndrome empty. Without at least cursory constaints, the existence of this rule type licenses virtually any arbitrary string to occur in the environment [#____#] or not to occur in the environment [#____#].

However, Kean makes some attempt to sidestep these criticisms by claiming that *phonological word* is "defined within universal linguistic theory" (1980a:358, cf., also, 1977:41, 1980b:251, 1981:197). This is less than reassuring. Universal linguistic theory in principle is a genetically encoded Grand Unification scheme for language; in practice, it is a realm of forms, appealed to with increasing frequency in work by MIT alumni and some of their instructors, where such things as island constraints and responsible definitions have residence. Kean is too recent a graduate to call this construct by its 1965 name, LAD (Language Acquisition Device; see Chomsky [1965:52]), but her Broca analysis has spanned two descendants: in her first paper (1977:40), ~~it~~ is LRCS (Language Responsible Cognitive

Structure; see Chomsky and Walker [1978:15]); in her recent work, it has the more up-to-date title, UG (Universal Grammar; see Chomsky [1982:7]). Whatever its label, Kean (1981:197) echoes Chomsky in assuring us that the "theory of universal grammar is an empirical hypothesis". This is true, but it is not a hypothesis subject to verification before another acronym is much better understood, DNA. For the moment, a more useful characterization comes from Dennett (1980:19): "passing the buck to biology".

There are a number of other problems with her work – such as the claim that the English plural is derivational, and the systematic misuse of *clitic* as a foil to *phonological word* (see Harris [1983] for a more detailed critique) – but the issue of principal concern here is the participation of *phonological word* in stress assignment. The following synopsis should demonstrate that the theory is no less problematic here:

(a) If a word is a N, V, Adj., or Adv., assign # boundaries to both sides of it (pp. 21, 24, 29).

(b) Assign stress only to words flanked by # boundaries (p. 24).

(c) "A phonological word [is] the domain over which the assignment of stress [takes] place" (p. 31). (Klosek [1979:64];the page numbers refer to Kean [1977]).

In short, the concept is "ultimately founded on lexico-syntactic criteria" (Klosek [1979:64]). More specifically: *phonological word* is isomorphic with terms such as *major category* (defined syntactically), *major lexical item* (defined lexically), *content word* (defined semantically), and *open class word* (defined in neutral, set-theoretic terms). It looks very much like Kean (1977) has coined a term, appropriated a well-established linguistic notion, and built a theory around it. Further, the phonological word / nonphonological word dichotomy "is not a stress / stressless distinction universally" (Kean [1979:78, cf., also, 1977:34, 1980a:358, 1981:192]).

Still, the implication remains that in some non-universal, language-specific cases, stress is criterial, and while there are some later hedgings, it is clear in the first paper that English is one of those cases: *phonological word* is "defined in English as the domain over which the assignment of stress [takes] place" (1977:21). Despite the manifold complications, and confusions, of Kean's phonological proposal, this statement, in concert with the claim that Broca victims reduce speech to a string of phonological words, makes her proposal dovetail quite smoothly with the research outlined above and the general hypothesis of this thesis. Moreover, the difficulties posed by polysyllabic and compound prepositions for a simple functor loss theory are avoided in her account. Her phonological component merely flanks all prepositions with the #-boundaries and then employs readjustment rules to erase those on either side of monosyllabic ones (Kean [1981:190]). (This procedure marks another point of departure from SPE, which does not assign #s to any prepositions - Chomsky and Halle [1968:366, convention

115].[14]) The solution is very *ad hoc*, but it works, and it effectively reduces her claim to '(English) Broca victims exploit stressed words and have difficulty with nonstressed words'.

This is essentially the hypothesis my replication of Heilman and Scholes (1976) is designed to test. But it is important to notice that the experiment does not, and could not, test Kean's theory. For one thing, stress is incidental to her proposal. But more significantly, there is no conceivable way to test her claims. Her theory rests on the amorphous and wholly unverifiable notion, *phonological word*.

2.3 Conclusion

There is one further point of contact between Kean's argument and the argument of this thesis, however: stress is also incidental here. But it is incidental in a quite distinct way. For Kean, stress assignment is a reflex of the phonological word category (in English), and consequently a "diagnostic" of that category (1977:34); apparently it is one of many such diagnostics, though no others are ever specified. But the mechanisms of stress assignment, and nebulous categorizations of lexical items, appear to be well beside the point in the Broca comprehension problem. The crucial factor – following from a careful, if lengthy, sifting of the empirical studies – looks to be the simple acoustic properties of the speech signal. That is, relatively long and loud words with proportionately more distinct f_0s are processed by Broca victims better than shorter, quieter words with less distinct f_0 values. The critical acoustic notion is *salience*.

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[14] Lapointe (1983) also notices her use of readjustment rules differs from SPE; however, he does not see that as a problem.

Of course, *salience* has not traditionally been anymore well-defined in linguistics than, say, *phonological word*, but it can be localized outside of an arbitrary formalism in a way that the latter cannot, and it can be quantified. For present purposes, it is identified with a direct increase of duration, amplitude and distinctiveness of fundamental frequency. The items altered in my replication of Heilman and Scholes (1976) cannot be accurately described as receiving more stress, since stress as a theoretical concept is also associated with "a certain degree of vagueness". Stress is relative, definable only with reference to nonstress (in turn obviously definable only in terms of stress); the problems of definition are very thorny. But the stimulus modifications can be described as increasing salience – something that could be tested by, for instance, masking experiments.

More to the point, there is already evidence increased duration, and to a lesser extent increases of amplitude and f_0, facilitates perception for aphasia victims, the population of concern here. The temporal evidence is most concrete. Efron (1963) and the several researchers following his lead convincingly demonstrate that increased time makes pure tones, arbitrary complex tones, speech sounds, and words significantly more salient for aphasics. These findings are particularly important because they strongly suggest that some of the perceptual difficulties of aphasia may be epiphenomenal (Efron [1963:418]). In performance terms, they signal the presence of "a certain kind of 'noise' or defect in the auditory system of aphasic patients even at a stage preceding speech comprehension" (Sasanuma *et al.* [1973:72]). The results are also quite robust, following from a number of paradigms with a wide range of stimuli.

The temporal work more directly concerned with comprehension is less robust, but again a diversity of stimuli and paradigms have generated quite consistent results – and the conclusion these results motivate is that the influence increased time has on speech perception also extends to speech comprehension. The indications are not as uniform. Some researchers claim to have uncovered counter-evidence (Blanchard and Prescott [1980], Riensche *et al.* [1983]), and virtually all studies that break down performance by individual isolate a few who improved only slightly, or did not improve at all. But most of the studies produced convincing significance levels for better performance under their expanded conditions. All the general tests demonstrated comprehension to improve nontrivially as a function of increased time, though some were not reliably controlled (e.g., Sheehan *et al.* [1973]), and the Token Tests were for the most part just as straightforward. The TT paradigm is especially important, of course, since Broca victims do rather well with generalized comprehension probes, like the Minnesota Test, while averaging scores indistinct from those of Wernicke patients on the more pointed Token Test. And all temporal modifications of De Renzi and Vignolo's (1962) paradigm, even those that claimed to have found counter-evidence to expansion facilitating comprehension, found prominent subgroups that benefitted from increased duration. Poeck and Pietron (1981), the most extensive study, found the most stable results. Further, the two experiments that looked at the data in terms of task gradation (Parkhurst [1970], Marckworth [1976]), found that variable performance decreased as the tasks became more difficult: the harder the task, the more visibly temporal expansion improved performance. Since message complexity is a type of information noise, increased duration here can be seen as a direct manifestation of salience, resisting noise in the stimuli.

An interesting sidenote, parallel to the complexity result, also comes out of the Token Test experiments. Liles and Brookshire (1975) produced numbers indicating that aphasia involves memory limitations, and that these limitations interact with temporal expansion. In particular, they found that interpolated silence late in the stimulus sentence only improved comprehension at $\alpha=0.25$. More specific explorations of short term memory loss in Broca's syndrome, discussed in Chapter 4, have also found these deficiencies to be significant. This means that there is evidence of a particular type of noise in the auditory system – message degradation as a (partial) function of hardware storage restrictions – which lies outside any competence-defined comprehension models. It also means that certain kinds of expansion techniques, including at least late-interpolated silence, do not sufficiently boost message salience to resist that noise.

Experimental research into the saliency for aphasics of the remaining two properties – amplitude and f_0 – is not near so straightforward as the temporal work, but the final indications are similar. Although there is no precisely focussed evidence that increases of amplitude or frequency benefit aphasic comprehension, a number of factors conspire to that conclusion. First, with specific reference to amplitude, there is the clinical impression that comprehension improves with "increase[s to] the loudness of the auditory signal until an optimal level is found" (Darley [1976:1]; c.f. Schuell *et al*. [1964:339]). Second, it is a well-documented fact of psychoacoustics that all three aural dimensions impinge upon the perception of one another. For instance, discrimination of pitch, a psychological notion most closely associated with f_0, differs as a function of signal duration and signal amplitude; and loudness, a notion associated closely with amplitude, is perceived differently at different durations and frequencies. Moreover, time and pitch have an especially close relationship, since a long tone contains more pitch periods than a shorter one of the same frequency. So the temporal experiments which uniformly expanded the signal (e.g., Marckworth [1976], Pashek and Brookshire [1982]) bear indirectly on the saliency of amplitude and fundamental frequency (since uniform duration increases are also perceived as amplitude and frequency shifts).

Third, and of more direct relevance, the stress experiments also bear on amplitude and f_0 saliency. They demonstrate convincingly that victims of aphasia are not peculiarly impaired for stress detection: Goodglass *et al*. (1967) showed retained stress perception with repetition tasks; Laughlin *et al*. (1979) and Swinney *et al*. (1980) showed the same with recognition tasks; the one study with opposite conclusions, Baum *et al*. (1982) is so fraught with methodological problems that it cannot be taken seriously. This finding, then, is also quite robust.

Fourth, this preserved stress perception clearly influences comprehension: Blumstein and Goodglass (1972) found that aphasics could correctly identify minimal pair members distinguished only by lexical stress; Pashek and Brookshire (1982) found improved comprehension with selective use of emphatic stress. And fifth, since Pashek and Brookshires' stimuli were paragraphs of more than 90 words, there are indications in their results that stress facilitates message retention – that it combats the noise of memory limitations. In short, there is a substantial amount of independently gathered data that demonstrate the use of stress facilitates aphasic comprehension – that messages are more salient for aphasia patients when they involve extra stress – and stress involves increments of both amplitude and fundamental frequency.

Since stress involves increments of both, there is no evidence that bears exclusively on either amplitude or f_0 saliency for aphasia victims. In fact, since stress involves durational increments as well, there is no evidence that even isolates the complex of these two factors, and Pashek and Brookshire's failure to locate a rate x stress interaction may suggest only increase of duration is critical. But that conclusion is far from self-evident (Pashek and Brookshire [381] reject it), and there is sufficient indication in the stress work that the amplitude and f_0 components cannot be dismissed out of hand. In any event, it is very clear from the temporal and stress experiments that language can be made more resilient to noise for victims of aphasia.

The corollary conclusion from the function word experimentation is that certain words are, for wholly acoustic reasons, more noise resistant than other words. Specifically, the well-controlled experiments involving short, unstressed functors, like that of Heilman and Scholes (1976), found a peculiar failure among their Broca subjects to understand the stimuli fully; the well-controlled experiments involving longer, stressed functors, like Mack (1981) uncovered no such failure. That is, the function word research – though for the most part designed to do precisely the opposite – supports the saliency hypothesis with as much force as the expressly sonic research.

The most immediate implication of this support, and of the functor studies in general, is that Broca's syndrome does not entail a uniform loss of function words. While no researchers seriously entertain this possibility, except perhaps Scholes (1978), functor loss is the most frequently cited tendency of the comprehension side to Broca's syndrome. But, since function word difficulties are not uniform, there is very clearly something beyond a functor-contentive distinction that generates results like those of Heilman and Scholes, and that underwrites the perceptual problems of Broca victims. Certainly there are formal and rhetorical manouevres available to linguists who seek to elevate that tendency to definitional status – such as Zurif and Kean – but a more useful research program is to try to discover the dimensions of that tendency. The important question is not 'what (syntactic or phonological) class of words is unavailable to Broca's aphasics?' but 'what characteristics of that class hamper its availability to Broca's aphasics?'

There is some evidence, not considered to this point, that an important characteristic of the class is semantic. For instance, Gardner and Zurif's (1975) aptly titled paper, "'Bee' but not 'be'", reports that Broca victims' accuracy decreased and response times increased on a simple one word reading task, as the words departed from an ideal the authors termed 'picturability'. The subjects were most reliable and prompt for the lexical items most evident in their production, concrete nouns, and least reliable for abstract items. This result held even when the nouns were relatively long and phonologically complex (e.g., *hydrant*), and the abstract items were short, easily articulated items (e.g., *at*). Further, the subjects' ability to read function words was no more impaired than their ability with other nonconcrete categories (notice that this in itself argues against a syntactic categorial deficit). Recall that Friederici (1982) also found her subjects to have more difficulty with 'syntactic' prepositions than with the more concrete 'semantic' ones. Similar findings come from a case study reported by Andreewsky and Seron (1975) of a French Broca patient who was incapable of reading aloud items like *car* and *or* when they were constrained to functional roles (= *because* and *now*), but had little difficulty when there was either a content constraint (= *bus* and *gold*), or no constraint at all. (When there were no constraints, it is probable he was 'reading' the content denotation; e.g., unconstrained *or* was read paralexically as *diamant* [*diamond*].)

But *next to* is not, in principle, any more dense semantically, or more picturable, than *on*; yet Mack (1981) found that the former was reliably understood by his subjects, while Seron and Deloche (1981) found their subjects to disregard the latter in favour of pragmatic strategies. So, while semantic weight most probably accounts for some of the variance in the Broca comprehension results, it is not sufficient on its own.

Goodglass (1976:253) defines saliency for agrammatics "as the resultant of information load, affective tone, and increased amplitude and intonational stress". The definition is vague, and problematic for research purposes. It is also too loosely formulated; since, for example, the "information load" on *the*, as the sole disambiguator of argument structure in Heilman and Scholes' paradigm, is very heavy. But it does recognize a semantic component to the disorder, and consequently is a more complete identification of the comprehension problems than the definition of *salience* adopted by this thesis.

For the purposes of this study, *salience* refers only to the last two factors Goodglass isolates, the acoustic factors. The reasons behind this restriction have been rehearsed several times to this point: (1) it localizes the term as an exclusively performance variable; (2) it is very well supported by empirical research along two fronts; and (3) it licenses a very straightforward, testable hypothesis.

3. Experimental Procedures and Results

3.1 Introduction

The central hypothesis of this thesis is a simple one: that Broca's aphasics suffer a significant portion of their comprehension difficulties because of a decrease in, or interference with, acoustic processing ability. In specific, they unreliably process short, unstressed function words due to some complex of the following three signal characteristics: brief duration, low amplitude, and indistinct f_0. (This proposal does not rule out the possibility that another significant portion of these difficulties might follow from other characteristics of functors, such as low semantic weight.) The Heilman and Scholes (1976) replication around which this thesis is organized investigates that hypothesis: if it is correct, altering the acoustic dimensions of a normally short, unstressed functor (*the*) should affect Broca's aphasics' facility in processing a sentence whose interpretation hinges on it; if *the* is made longer, louder, and more distinct in pitch, they should comprehend the sentence better.

Nine Broca's aphasics and nine neurologically sound nonaphasics were enlisted in the test. They were each scored on their responses to 16 sentences, equally divided by two conditions (NORMAL and SALIENT). The eight sentences of each condition were comprised of the same four pairs, distinct solely in the post-verb location of *the*, as illustrated by the contrast of 1 and 2:

1. The man showed her baby the pictures.
2. The man showed her the baby pictures.

The NORMAL condition was a normally intoned (recorded) reading of the sentence; the SALIENT condition was the same reading except that the second *the* was removed and a longer, louder, more distinct *the* token was substituted.

The subjects indicated their comprehension of the sentences by selecting the line drawing, from an array of four, that they thought best corresponded to the meaning of the sentence. A typical array for sentence 1, in either condition, would be a depiction of sentence 1, a depiction of sentence 2, and two depictions of actions entailing other major lexical items (e.g., a man showing some girls' hats to a woman and a man showing hats to some girls). Systematic selection of the correct depiction was deemed to reflect good comprehension; random selection of either the correct choice or the choice involving similar major lexical items was deemed to indicate comprehension of the content words, but a deficiency with functors; and random selection among all four (which did not occur) would have been deemed to indicate a general lack of comprehension.

In the opposition of the two conditions, then, significantly more correct responses to the SALIENT sentences was deemed to reflect better comprehension as a function of better perception. Significantly more correct responses in the NORMAL condition (non-occurring) would have been deemed to indicate that the acoustic alterations had caused some form of perceptual interference. Lack of significance would have indicated that acoustic perception (at least at the tested levels) has little bearing on Broca patients' functor difficulties.

The remainder of this chapter reports in detail on the subjects, methods, and the results of the experiment.

3.2 The Experiment

3.2.1 Subjects

3.2.1.1 General Description

The subjects were nine victims of left anterior CVAs diagnosed as Broca's aphasics, and nine nonaphasic controls matched for gender and age (±5 years, with one discrepancy of +7 years). The aphasic subjects were all enrolled in language intervention programs. Five of the nonaphasics were recruited through several hospitals, and four were unhospitalized volunteers (there was no attempt to control for institutionalization). Table 3.1 gives descriptions of the members of both groups. Each pool is comprised of five males and four females. The aphasics had a mean age of 65.33 years ($s^2=11.4$), and a mean education level, where available, of 10.94 years ($s^2=3.55$). The controls had a mean age of 66.89 years ($s^2=10.6$), and a mean education level of 10.67 years ($s^2=3.87$). All subjects had good vision – good acuity with no field deficits – though several

Table 3.1

GENERAL SUBJECT DESCRIPTIONS

Subjects	Age	Gender	Education (Years)	Visual Acuity	Auditory Acuity
Aphasic					
RS	80	female	12	good	fair
FD	77	male	n/a	good	fair
JC	77	male	10	good	fair
HT	70	male	4	good	fair
WP	65	male	12	good	n/a
AM	58	female	16	good	good
DS	58	female	12	good	good
JP	56	male	8/9	good	good
JF	47	female	13	good	good
Nonaphasic					
RW	79	male	7	good	poor
GW	77	female	12	good	good
EB	76	male	5	good	fair
MU	74	male	6	good	fair
SC	70	male	12	good	fair
LP	63	male	12	good	fair
LL	57	female	12	good	good
PN	55	female	17	good	good
MB	51	female	13	good	good

wore corrective lenses, and both groups had relatively good hearing (as defined below). All were right-handed native speakers of English residing in one of the two westernmost Canadian provinces.

3.2.1.2 Aphasic Subjects

The aphasic subjects were recruited through the good offices of several Vancouver hospitals. They were more or less evenly distributed between out-patient and in-patient status (five outpatients, four inpatients), though none had been institutionalized for much more than half a year and, barring future complications, all but one severely hemiplegic man (FD) were scheduled for discharge. As above, all had suffered left anterior CVAs, and all had been diagnosed by registered therapists as Broca's aphasics. Table 3.2 gives a description of this group in terms of variables specific to their disorder. Their mean postonset time was seven months (s^2=5.27). Most had some degree of dysarthria, most had some degree of apraxia, and most had some right side paresis or paralysis. All were very eager to participate and in good spirits, and only one gentleman (FD) was less than fully alert. He began the session sharp and cheerful, but quickly fatigued. It perhaps would have been wiser to schedule two sessions with FD, but exigencies would not permit more than one. (However, while the fatigue slowed his responses and required repetitions, it did not appear to affect the accuracy of his selections, or perhaps increasing comfortableness with the the task counterbalanced his tiredness, since his first eight and his last eight responses contain exactly five errors each, and his two content errors were committed within the first four trials.)

At this level of description, the patients look quite homogeneous, and in fact they were selected with a series of decision rules that aimed at relative uniformity. Several other patients were also tested – patients whose responses would *strengthen* the significance results reported below – but they were excluded from the experiment for violating one criterion or another. One patient was excluded because his

Table 3.2

APHASIA SPECIFIC DESCRIPTIONS

Subjects	CVA Postonset	Apraxia	Dysarthria	Hemiplegia / Hemiparesis	Output / Comprehension Severity	Session Mood / Alertness
RS	11 months	none	mild	R, mild	mild / mild	friendly / alert
FD	19 months	mild	moderate	R, severe	severe / moderate	tired
JC	2 months	none	mild	R, mild	moderate / mild	eager / alert
HT	2 months	mild	none	none	moderate / mild	friendly / alert
WP	7 months	mild	severe	R, moderate	severe / moderate	eager / alert
AM	7 months	mild	none	R, moderate	moderate / mild	friendly / alert
DS	5 months	moderate	none	R, mild	moderate / mild	eager / alert
JP	5 months	mild	moderate	R, moderate	mild / mild	friendly / alert
JF	5 months	mild	mild	R, mild	mild / mild	friendly / alert

productive symptomology was inappropriate, though lesion site was left anterior. Two more were excluded because, while symptomologies were appropriate, lesion site information was unavailable. Two more were excluded because, though lesion sites and symptomologies were appropriate and they were fluent in English premorbidly, it was not their native language.

However, despite this screening mechanism, there were several variables revealed in a review of patient files which introduced unwelcome diversity, and which have not been tabulated. For instance, AM is bilingual, with a fluent (premorbid) knowledge of French. JC had suffered at least three CVAs, dating back to 1979. JP's CT scan showed some frontal lobe atrophy and his social report revealed that he had been a "very heavy drinker" prior to his CVA (he had quit drinking entirely postonset after discovering that three beer made him "feel awful"). A therapist who worked with JP but did not originally diagnose him suggested that his Broca classification was incorrect and that he may only have been strongly apraxic (however, she made this observation after a discussion of his near flawless experimental performance, and it would have been somewhat dishonest to exclude his data at that point). Some of the CVAs were embolic, while others were aneurysms, and several patients were receiving hypertension or anti-coagulant medication.

I report these subject complications here not with the intention of mitigating the validity of my results, though they may be interpreted in that way, nor with the intention of claiming the results are all the more powerful for visibility through the haze of this limited heterogeneity, though again, the argument is available. I report them out of the conviction that the best research attempts comprehensive reporting, especially when it is presented in a work with the scope of a thesis, and alerts caution in the reader. But it is also worth noting that such things as medication, alcohol consumption, and second language facility varied in the control group as well (in an uncontrolled way), and no knowledge was available as to the CNS fitness of its members. Perhaps some normals also had cortical atrophy or some other damage. Nor, to the best of my knowledge, is there documentation on different behaviours consequent to blockage than consequent to hemorrhage, and the specific medications taken by some patients are not known to affect cognition. (Interaction among these variables is yet another story.)

There is reason for caution; there is no reason for despair.

3.2.1.3 Ethics

The study presented no danger to the subjects. Each was told (s)he could terminate the session as (s)he saw fit, and each signed a release form to that effect (including one subject whose therapist thought his writing ability was too severely impaired to sign), and all subjects who were more than mildly impaired were tested in the presence of their therapists. The patients of Vancouver General Hospital signed a special in-house form. All others signed the form given in Appendix B.

The study was screened and approved by The Department of Linguistics Committee on Ethics in Human Research and by a research committee at each hospital which provided subjects.

3.2.2 Materials

3.2.2.1 Stimulus Items

Sixteen stimulus sentences were used, of approximately seven words each, in two conditions: eight were of NORMAL intonation; the other eight were the same sentences, digitally altered to replace the key functors with more SALIENT tokens. The eight sentences are given here as 1 - 8 (1 and 2, given above, are repeated for convenience):

1. The man showed her the baby pictures.
2. The man showed her baby the pictures.
3. The man showed the boys the horse shoes.
4. The man showed the boys' horse the shoes.
5. The man showed the girls the bird seed.
6. The man showed the girls' bird the seed.
7. The man showed her girls the hats.
8. The man showed her the girls' hats.

Sentences 1 - 8 were recorded by a trained male speaker with an f_0 just slightly under the normal adult male value of 100 Hz, in a sound-treated booth, using a Sennheiser MD 421N microphone and a Teac A-7030 open reel tape recorder. He read the sentences with normal intonation, at a deliberate pace, with precise enunciation; the speech rate was approximately 190 wpm. The list he read from was randomized and included some distracting sentences which were not used in the experiment, and he was not told anything about the experiment until after the recording session. During the same session, he also recorded eight isolated two-word noun phrases, all beginning with *the*, followed by a noun corresponding to those of the key noun phrases in 1 - 8. These phrases are given here as P1 - P8:

P1 The baby
P2 The pictures
P3 The horse
P4 The shoes
P5 The bird
P6 The seed
P7 The hats
P8 The girls

Since the acoustic dimensions of short, isolated phrases are more pronounced than those of the same phrase within a sentence, this procedure resulted in tokens of *the* which were longer, louder, and more distinct in pitch than their counterparts in 1 - 8. The advantage of recording phrases – as opposed to recording the functor as an isolated lexical token – was twofold: first, it limited the duration and amplitude to manageable bounds, since functors read as part of a lexical list are exceedingly loud and long relative to functors in context; and second, it controlled for both consonantal and vocalic coarticulation variables.

Several tokens each of 1 - 8 and P1 - P8 were recorded, and the best were digitized on the Alligator system

(Stevenson and Stephens [1978a, 1978b]) on a DEC PDP-12A minicomputer, with a sampling rate of 10kHz and an effective frequency response after filtering of 68Hz - 6.8kHz. Using a visual editor precise enough to display each individual data point, the sentences were then broken into three segments and stored separately. Figure 3.1 illustrates the segmentation, for sentences 1 and 2.

[the man showed her HEAD1] [the THE1] [baby pictures TAIL1]

[the man showed her baby HEAD2] [the THE2] [pictures TAIL2]

Figure 3.1: Stimulus sentence segmentation

The phrasal tokens were also edited: the nouns were gated off and erased, and remaining signals were stored with mnemonic tags linking them to their counterpart sentences. For example, the signal taken from P1 was labelled "PTHE1", from P2, "PTHE2", etc. For each sentence type, then, there were four signals stored – a HEAD, a TAIL, a THE, and a PTHE – each suffixed with an identifying numeral. The NORMAL condition sentences were prepared simply by concatenating HEADs, THEs, and TAILs with the same numeric suffix, reconstituting the original sentence. The SALIENT condition consisted of corresponding HEAD, PTHE, and TAIL concatenations. (The concatenation program, RPLAY, is given in Appendix C.)

This procedure resulted in eight pairs of sentences, distinct only in the acoustic dimensions of the key function words (and in the parameters affected by those dimensions; e.g., since all PTHEs are longer than all THEs, the SALIENT sentences were marginally longer than the NORMAL sentences). Table 3.3 lists the durations, amplitudes, and fundamental frequencies of all 16 *the* tokens employed in the experiment. (The measuring programs, MEASDA and MEASP, are also given in Appendix C.) The mean duration for the NORMAL *the* tokens used is 87.8 msec, and the mean amplitude (area) is 878.75 RMS; the mean duration for their SALIENT counterparts is 226 msec, the mean amplitude 2,266.25 RMS. On the average, then, the SALIENT tokens are over 2.5 times as long and almost 10 dB greater in intensity than the NORMAL *the*s. The amplitude range of the phrasal tokens is also greater, since the mean maximum and minimum values are higher and lower respectively than for the NORMAL statistics, while the variance for both is less.

The differences in fundamental frequency are not as pronounced: the average f_0 of the NORMAL tokens is 97.75 Hz; the SALIENT versions are somewhat lower, at 93.5 Hz. This is a clear departure from the parallels to stress in the other two dimensions. Stressed vowels are longer and louder than the same vowels unstressed, and the SALIENT *the* tokens are longer and louder than the NORMAL sentential tokens.

Table 3.3

NORMAL AND SALIENT *THE* MEASUREMENTS

	Duration(msec)	Amplitude(RMS)			Pitch(Hz)
		Max/Min	Area	Average	
THE1	102	767/190	1,567	497.970	102
THE2	120	624/333	429	502.405	88
THE3	93	794/247	1,311	496.737	96
THE4	53	630/324	626	500.948	94
THE5	97	665/319	685	501.962	109
THE6	63	676/268	1,077	500.379	95
THE7	106	615/311	746	491.383	104
THE8	69	704/322	589	498.513	94
$\bar{x}$	87.87	684/289	878.75	498.787	97.75
s^2	23.48	66.6/50.2	397.33	3.588	6.73
PTHE1	257	782/226	2,948	498.296	94
PTHE2	242	750/248	1,725	496.148	95
PTHE3	248	796/256	3,294	496.248	94
PTHE4	193	724/280	1,843	498.187	89
PTHE5	257	782/226	2,948	498.296	94
PTHE6	178	688/286	1,410	498.523	95
PTHE7	230	771/220	2,067	498.512	96
PTHE8	203	801/300	1,895	498.886	91
$\bar{x}$	226	761/255	2,266.25	497.887	93.5
s^2	30.71	38.9/30.6	693.71	1.064	2.3

But stressed vowels are also higher in pitch than unstressed vowels, and the SALIENT versions have lower f_0s than the NORMAL condition *thes*. Moreover, as the respective variances indicate, this difference is not uniform. THE2, for instance, is higher in pitch than PTHE2, and several other pairs are either equal or very nearly so.

As the discussion of stress in the previous chapter points out, however, this does not pose any theoretical difficulties for the experiment, since any effect the stimulus manipulations had on stress contours was incidental. The modifications were carried out solely to effect an increase in salience, not to alter stress patterns, and the salience of f_0 was in fact greater in the phrasal tokens. It is well known that boosting duration or amplitude, or both, improves frequency discrimination (e.g., Ainsworth [1976:24-26]), and the SALIENT versions are higher in both dimensions. But the number of periods was also substantially greater for the phrasal tokens than for the sentential tokens. The SALIENT condition *the*s averaged seven periods per token, with a range of 5 - 8, while the NORMAL tokens averaged only 2.25 periods and had a range of 1 - 3.

The 16 sentences were assembled in a unique randomization for each subject (RPLAY), filtered through a Rockland 1520 Dual Hi/Lo Filter with cut-offs of 68Hz and 6.8kHz, and recorded on a TC K55II Sony Cassette recorder. Each randomization consisted of 18 sentences, with the first two and the last two being identical (the first two responses were not recorded, and were used to familiarize the subjects with the task).

Figure 3.2 contains linear scale contour spectrograms, marked at 1000 Hz intervals, prepared on a Kay Elemetrics Model 7800 Digital Sona-Graph®, of both the NORMAL and the SALIENT versions of sentence 1: *the man showed her the baby pictures.*[17] For ease of comparison, they are aligned

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[17] For readers unfamiliar with sound spectrograms, they represent pockets of energy as they are distributed through an acoustic waveform. All three dimensions are represented: a spectrograph converts signal duration into spectrogram length, frequency into height, and amplitude into density of shading. The spectrograms of Figure 3.2 represent from zero

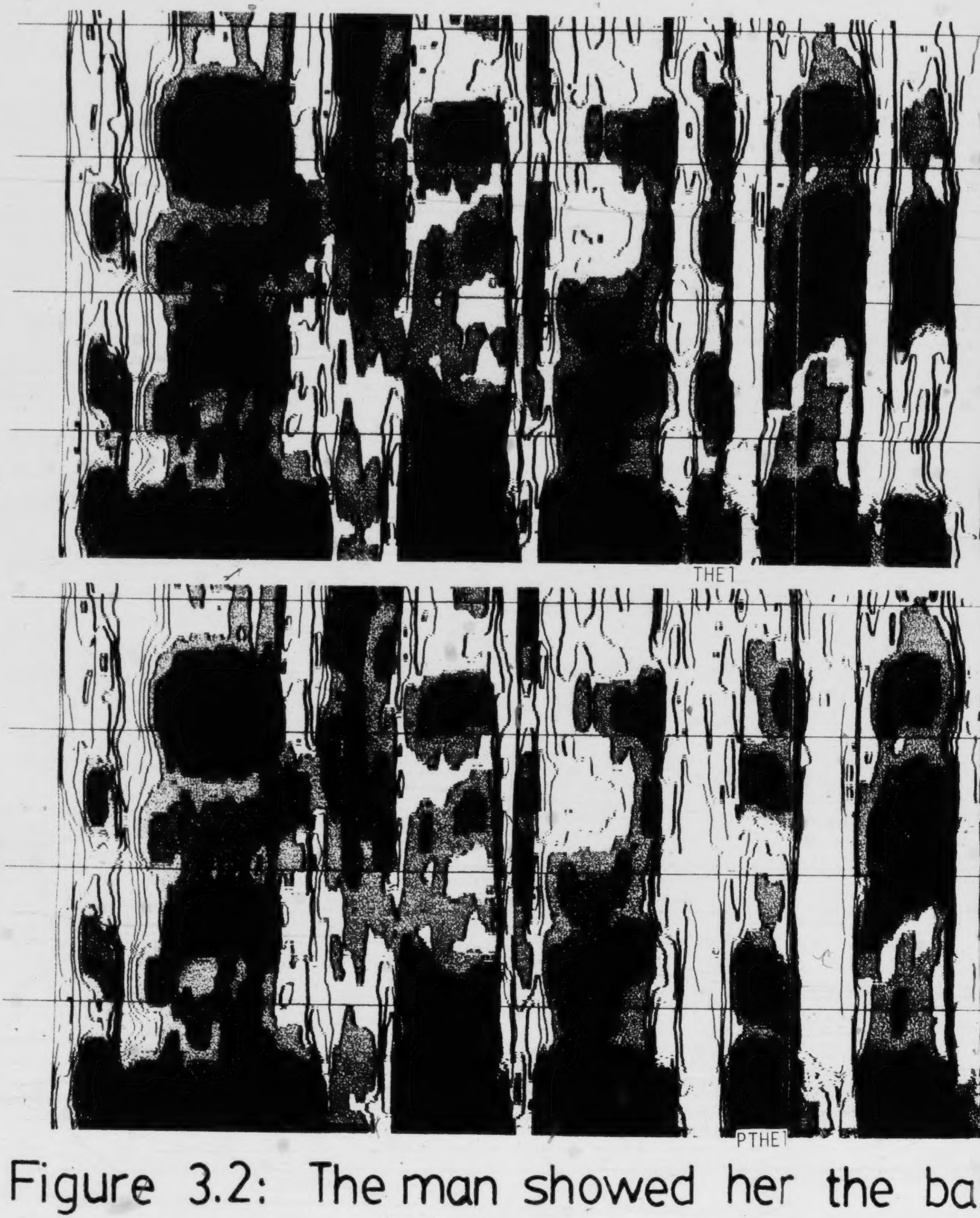
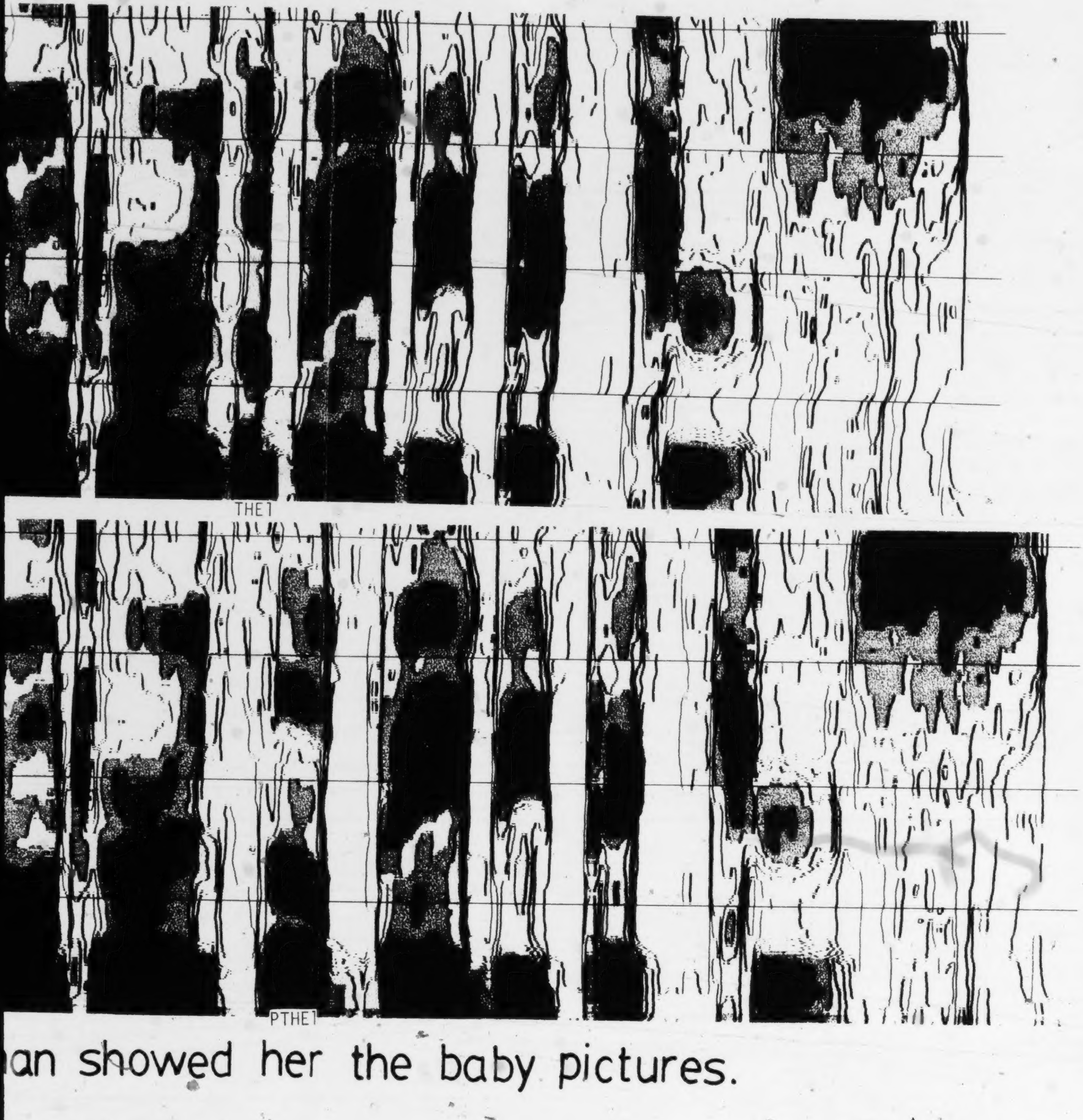


Figure 3.2: The man showed her the ba



ıan showed her the baby pictures.

2 of 2

sentence-initially, NORMAL over SALIENT, and the key functors are labelled. (The remaining seven sentences are represented spectrographically, again with NORMAL over SALIENT, in Appendix D.)

The differences apparent or tacit in Table 3.3 are more explicit in the spectrograms. It is evident that the SALIENT condition sentences are longer overall than the NORMAL sentences, and it is evident that the differences in duration are a product solely of the functor substitution. In addition to the temporal differences, the spectrograms also illustrate the higher amplitudes of the SALIENT *the*s, through the greater area of the dark contours, and show their much better defined vocalic nuclei. F_2 is especially distinct, in terms of both its frequency and its labial transition. Notice, however, that even the NORMAL *the* in Figure 3.2 is considerably more visible (therefore, more audible) than the token preceding *man*; indeed, the latter does not have a presence distinct from the noun that follows it. This does not, as it might seem to, indicate that the NORMAL version is in fact abnormally well defined for an average sentential functor. While the speaker was a professional and was instructed to enunciate clearly, the differences between these two tokens have less to do with

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[17](cont'd) to (approximately) two seconds in duration, read left to right; from zero to just over 4000 Hz, read bottom to top; and from zero to about 800 RMS, read from light to dark. Therefore, the dark contours indicate relatively intense energy concentrations, whose frequency and duration are identifiable both on objective scales and relative to other concentrations.

his clarity than with the sad fate of sentence-initial functors, which are squeezed out very rapidly because of the high subglottal pressure built up in anticipation of an extended word sequence.

A further point which should be clear from the spectrograms is that there are no marked discontinuities in the SALIENT sentences: no periods of silence flanking the substitution, no acoustic spikes, and no abrupt shifts in intensty or frequency. The Alligator did a very good job of gating off and splicing in the phrasal tokens, the speaker did a good job of controlling his amplitude, and the methodology took care of the coarticulation variables. That is, the SALIENT condition sentences sounded very normal. No one, aphasic or control, consciously detected the differences, and all were surprised to hear the study had an acoustic component.

3.2.2.2 Response Items

There were 16 four-drawing response arrays, each mounted on a 43 x 56 cm sheet of four-ply cardboard and covered with a glossless, transparent vinyl. The arrays were assembled from an original set of eight 21.5 x 28 cm line drawings, prepared by a University of Alberta Fine Arts student. The drawings were simple, representational and nondistracting. Each corresponded directly to one of the sentences above (1 - 8). For example, the drawing which corresponded to sentence 1 depicted a man holding two pictures of a baby out for a woman to look at. The eight drawings constitute Appendix E.

Since the original set was small, all the drawings recurred several times throughout the test, so the correct choice for one stimulus sentence showed up again as an incorrect choice for another sentence. Each array contained a correct choice, a "function" distractor, and two "content" distractors paired by lexical content (i.e., the two content distractors were the correct and function errors of another sentence).[18] The drawings were randomized within each array, so that positions of the correct response, the function error, and the content errors were not predictable. Table 3.4 illustrates the 16 arrays, which were ordered according to the stimulus randomization they were used with.

Two additional 43 x 56 cm sheets were also used, though these are not properly called "response items", since they were used in a pretest task. They consisted of drawings of the figures and props depicted in the experiment. The boys, the girls, the man, the woman, the baby, the horse, and the bird were on one sheet; the shoes, the horse shoes, the pictures, the baby pictures, the hats, the girls' hats, and the seed (beside another picture of the bird for reference) were on the other. These drawings were virtually identical to the those used in the action scenes of the task proper; in fact, the original drawings were cut out, pasted on a sheet, and photocopied to get this second set. The only difference is that the prop depictions (and the second bird

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[18] If the "function" and "content" labels are not clear, see subsection "Scoring", below.

Table 3.4

RESPONSE ARRAYS

BABY$_1$ HATS$_2$	BABY$_1$ BABY$_2$	BIRD$_1$ HATS$_2$	BABY$_2$ BIRD$_2$
BABY$_2$ HATS$_1$	HORS$_1$ HORS$_2$	BIRD$_2$ HATS$_1$	BIRD$_1$ BABY$_1$
BIRD$_2$ HORS$_2$	HORS$_2$ BABY$_2$	BABY$_1$ BABY$_1$	HATS$_1$ HORS$_2$
HORS$_1$ BIRD$_1$	BABY$_2$ HORS$_1$	HORS$_2$ HORS$_1$	HATS$_2$ HORS$_1$
HORS$_1$ HATS$_2$	BIRD$_2$ BIRD$_1$	BIRD$_2$ HORS$_1$	BIRD$_2$ BIRD$_1$
HATS$_2$ HORS$_2$	BABY$_1$ BABY$_2$	HORS$_2$ BIRD$_1$	HATS$_1$ HATS$_2$
HATS$_2$ BIRD$_2$	BABY$_1$ BIRD$_1$	HATS$_2$ BABY$_1$	HORS$_1$ HATS$_1$
BIRD$_1$ HATS$_1$	BABY$_2$ BIRD$_2$	HATS$_1$ BABY$_2$	HATS$_2$ HORS$_2$

Where (as in Appendix D):

BABY$_1$ = a man showing baby pictures to a woman
BABY$_2$ = a man showing pictures to a baby
HORS$_1$ = a man showing horse shoes to some boys
HORS$_2$ = a man showing shoes to a horse
BIRD$_1$ = a man showing bird seed to some girls
BIRD$_2$ = a man showing seed to some girls
HATS$_1$ = a man showing girls' hats to a woman
HATS$_2$ = a man showing hats to some girls

picture) were expanded by a factor of 50%, to fill out the sheet better.

3.2.3 Procedures

Each experimental session consisted of four procedures, ranging between 30 and 75 minutes in total, depending on the mood and severity of the subject. First there was an introductory period, followed by an auditory screening, a combination comprehension screening and task preparation protocol, and the test proper.

3.2.3.1 Introductory Period

The introductory period served two purposes. First, since I was seeing most of the subjects for the first time, it simply gave us a chance to meet – so I could learn a little about them, and they could become comfortable with me. Second, it introduced the subjects in a very general way to the task ("You will hear a sentence over this speaker [pointing], and I would like you to choose the drawing off one of these cards [pointing] that is the same as the sentence") – allowing them to decide about proceeding. Before moving on to the second part of the session, the subjects read and signed the release forms. For some subjects, it was necessary to read the form out loud and to draw specific attention to their option of withdrawing at any time.

3.2.3.2 Auditory Screening

The auditory screening was conducted with an audiometer, though make and model varied from hospital to hospital. Four different testers were involved, though testing frequencies remained constant. One aphasic subject was not tested at all, and several of the normal subjects were tested under quite adverse conditions. This was the least controlled area of the study.

WP, the untested aphasic subject, had an external hearing aid, so closed field testing could not be conducted, and the facilities for open field testing were unavailable. However, since his hearing was aided and since his responses patterned similarly to patients of comparable severity, this did not seem sufficient to exclude him from the study.

The remaining subjects were tested at .5, 1, 2, 4, 6, and 8 kHz, which extends below and above the filtering range of the stimulus sentences (0.68 - 6.8 kHz). If they could discriminate reliably at under 40dB,their hearing was judged "good", and under 60dB, "fair", and at any value greater than 60dB, "poor", though only one (normal) subject tested in this range, and only in one ear, at 6 kHz and above. The rather high values for the "good" and "fair" ratings are a product of the poor testing conditions, as the control of background noise was difficult; it should be noted that the stimulus sentences were played at a mean SPL of 72 dB (A), above the "fair" level.

Several subjects exhibited some loss at 8kHz, and some at 7kHz (tested only if they had difficulty at 8kHz), but high frequency loss is normal for their age group, and it occurred in both aphasic and normal subjects about equally.

3.2.3.3 Comprehension Screening and Task Preparation

The third procedure of the session involved a combination comprehension (and short term retention) screening test and task preparation. The subjects were shown two 43 x 56 cm sheets, discussed under "Response Items" above, which contained line drawings of all the figures and prop depictions used in the task proper. They were told:

> I want to show you some drawings.
> I want to make sure that you can

> recognize them.
> So I know they are good drawings.

Then each figure (prop) was pointed to in turn, with descriptions of the following sort:

> This is a man.
> This is a woman.
> This is her baby.
> These are hats.
> These are girls' hats.

When all the drawings had been introduced, the subjects were asked to point to them, with requests like:

> Show me the boys.
> Show me the boys' horse.
> Point to the pictures.
> Point to the baby pictures.

If an error was committed during this stage, it was first ignored and the request was made again later, after at least three intervening requests; that is, the first error was treated as a lapse in concentration. If the patient failed again, the request was repeated immediately, with heavier stress on the important word(s) (Show me the **girls**). A third failure resulted in a renaming of the confused figure (prop):

> No.
> That is the woman.
> These are the girls here [pointing].

Since the comprehension level of most patients was rated as mild, with only 1 moderate, and 1 severe'', and since the drawings were good ones, there were very few errors committed. In fact, only one subject (WP) reached

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'' In fact, FD was rated as "moderate-to-severe" by his therapist. The severe rating is reported herein because it most accurately reflects his (fatigued) state at the time of testing.

the third stage, and his trouble was confined to two items: he repeatedly pointed to the woman upon hearing "Show me the girls" (also upon hearing "Show me the woman"), and he expressed total bafflement at "Show me the girls' bird". His difficulties are not easily explained. The woman depicted is a youngish one and in the isolated drawings she is situated next to a baby, without the man to provide height reference; she could be construed as a girl. But the girls are clearly younger, and the plural marker was emphasized after his initial failure. With the redundancy of "*These* are the girls here [pointing]", he demonstrated comprehension and could thereafter point to them on request. As to "the girls' bird" - possession did not appear to be a problem, since both "boys' horse" and "girls' hats" elicited accurate and relatively prompt responses. Perhaps the contiguous /ɚ/ syllables were the problem. At any rate, following the explicit "This the girls' bird, here [pointing]", he grudgingly admitted that perhaps pointing to it was the correct response for "Show me the girls' bird", but would only carry out the request with a deliberately puzzled expression. (His responses for both the NORMAL and the SALIENT versions of sentence 6 were function errors. Apparently, the fuss made over "girls' bird" during this part of the session did not affect his responses. He did not recall the item any better because of the extra attention, nor did he commit any content errors.)

3.2.3.4 Task

The experiment proper was introduced with the following instructions:

> I will show you some drawings and
> you will hear a sentence. After you
> hear the sentence, please point to
> the drawing that is the same as the sentence.

The instructions were enunciated clearly and slowly, with a slight emphasis on "same". I was prepared to repeat these instructions as required, but the situation never arose (recall that the task had already been described briefly, and that they had just finished a "same as" picture-pointing task).

Each subject sat at a table. The arrays were stacked directly in front of each subject, with the speaker just beyond them. The first array was then presented very deliberately: I pointed to each drawing in turn, requesting that they look at it. Then the first sentence was played, and the tape recorder stopped. Following the response, I moved the first card to the bottom of the stack and exposed the second. When I was confident the subject had looked at each drawing of the second array, I played the second sentence, waited for the response, and then moved that card to the bottom. This cycle was repeated for the remaining 16 sentences, with the pace being determined by observing the subject. When the subject looked up at me, or stared off fixedly awaiting the next stimulus, or simply returned his gaze to the starting point, the next sentence was played.

If the subject delayed his response for 45 seconds or more, or if he requested it, the tape was rewound and the sentence replayed. Repetitions were not frequently required, but one subject (FD) needed two repetitions for two stimuli and single repetitions for three others before he was sure of his his reply. (The repetitions did not appear to affect his accuracy.)

The sentences were played on Telefunken Studio 1 HiFi receiver with the (notched) volume control set at 12. SPL measurements were done in one of the testing facilities in conditions approximating those of a test session with a Type 2120 Brüel and Kjær Frequency Analyzer, and the mean SPL was 72 dB (A) (s^2=1.31).

3.2.4 Scoring

The subjects heard 18 sentences and gave 18 responses, 16 of which were recorded. The first two sentences of all randomizations were identical to the last two, and were not scored. They were used to ensure that the subjects were comfortable with the task. (The question of whether this methodology had any effect is finessed for the moment, until the overall results are reported.)

Subject responses were scored to fall within one of three categories: (1) correct (pointing to the depiction of the sentence), (2) function error (pointing to the other depiction involving the same major lexical items as the correct choice), and (3) content error (pointing to either of the two depictions involving different major lexical items. However, while these labels are clearly appropriate, it is important to note that, respectively, they do not exclusively identify for any given trial (1) total comprehension, (2) confusion about function words, or (3) confusion about content. For instance, a subject who completely fails to understand the stimulus sentence would choose at random among the four pictures; he would not automatically commit a content error. In more explicit terms, only a subject who does not comprehend the content would commit a content error, but the same subject would have a 25% chance of selecting the correct drawing and a 25% chance of 'committing' a function error. Similarly, a subject who understands the content words but is unsure of the difference cued by *the* placement would be more apt to commit a function error, but would have an equal (50%) chance of making a correct response.

Still, this interpretive difficulty exists only at the level of the individual trial: there are, in principle, three distinct response patterns, illustrated by Table 3.5. Pattern 1 is, of course, the expected distribution of a nonaphasic population's responses. Pattern 2 is the expected distribution of a population whose members understand only the content words and not the relations between them (in specific, the argument assignment cued by *the*). Pattern 3 is the expected response pattern of a population whose members completely fail to understand all

Table 3.5

DISTINCT RESPONSE PATTERNS

	Correct	Function Error	Content Error
Pattern 1	N	0	0
Pattern 2	N/2	N/2	0
Pattern 3	N/4	N/4	N/2

(Where N = total number of responses)

stimulus sentences.

3.2.5 Results and Analysis

The responses were set up as three-dimensional matrices (subject x sentence x response) in two MTS files, one for the aphasic subjects and one for the nonaphasics. The files were read by a FORTRAN program (MEANS, Appendix F) which sums over subject and condition and calculates a series of means: the overall correct responses, mean function errors, and mean content errors; the same means under the NORMAL condition; and those means under the SALIENT condition. The output of the program for the aphasic subjects is given in Table 3.6.

A total of 144 aphasic responses were measured: 99 were correct, 41 were function errors, and 4 were content errors. Of the 16 responses per subject, an average of 11 were correct, 4.6 were function errors, and the remaining 0.4 were content errors. Breaking these figures down by

Table 3.6

MEAN APHASIC RESPONSES

Subject	Condition	Correct	Funct Error(s)	Cont Error(s)
RS	NORMAL	6	2	0
	SALIENT	6	2	0
FD	NORMAL	2	5	1
	SALIENT	4	3	1
JC	NORMAL	4	3	1
	SALIENT	8	0	0
HT	NORMAL	5	2	1
	SALIENT	7	1	0
WP	NORMAL	4	4	0
	SALIENT	6	2	0
AM	NORMAL	5	3	0
	SALIENT	5	3	0
DS	NORMAL	4	4	0
	SALIENT	6	2	0
JP	NORMAL	7	1	0
	SALIENT	8	0	0
JF	NORMAL	5	3	0
	SALIENT	7	1	0
$\bar{x}$	OVERALL	11	4.566	0.444
	NORMAL	4.667	3.0	0.333
	SALIENT	6.333	1.565	0.111
s^2	OVERALL	2.499	2.128	0.726
	NORMAL	1.414	1.225	0.5
	SALIENT	1.323	1.130	0.333

condition: out of the eight responses per subject in the NORMAL condition, an average of 4.7 were correct, 3 were function errors, and 0.3 were content errors; of the 8 SALIENT responses from each subject, the mean correct response was 6.3, the mean function error was 1.6, and the mean content error was just over 0.1.

These results indicate that the Broca subjects, on the average, have some difficulty with comprehension tasks keyed to functors, but do not have much difficulty with the content of simple nouns. In short, they substantiate the recent work of investigators such as Zurif, Caramazza, Goodenough, and others. They also replicate the effect discovered in Heilman and Scholes' original experiment.

However, there is a little more to the story. The inclusion of a further condition in the experiment resulted in better performance than Heilman and Scholes found. Specifically, the Broca subjects did better with sentences whose interpretation hinged on acoustically boosted functors than with those hinging on their unaltered counterparts. For the NORMAL sentences, they responded very close to chance level (Pattern 2 in Table 3.5). But under the SALIENT condition, they made half as many function errors, and a third as many content errors (though there were very few of this class overall), resulting in a 36% improvement — substantially better than chance.

There is some variance obscured in these means. Notice, for instance, that neither RS nor AM showed improvement and that AM was close to chance in both conditions, while JC's improvement was 100%. But such is the nature of averaging, and also in the nature of averaging is the ability to reveal trends. The trend revealed most clearly in these statistics is that the functor comprehension of these subjects improved with increased salience.

There is little point giving a table like 3.6 for the normal subjects, or pursuing a discussion of their averages, since their performance was virtually flawless: their error means are all close to zero, and their condition means are indistinct; they responded consonant with Pattern 1 of Table 3.5 above. Notice, however, that although there is no way with these results to distinguish normal performance in one condition from that in the other, it does not make sense to say functor salience has no effect on nonaphasic comprehension: there might very well be a ceiling effect in operation. This issue could be addressed by fatigue, or distraction, or masking studies, but it has not been confronted by the current experiment.

The question suspended above – whether the procedure of repeating the first two tokens of all randomizations as the last two and scoring only the repetitions had any effect on the data – is not much easier to answer now. The vastly unequal sample sizes (2 tokens against 14, or 18 measures against 126) makes formal statistical analyses unavailable, and the luck of the draw determined that the two conditions would not even be equally represented in the final pairs. Of the 18 randomization-final responses, five were to NORMAL stimuli and resulted in three correct responses with two function errors; 13 were SALIENT, with 12 correct against only 1 function error. There were no content errors. On the surface, this looks like there was no effect at all for the NORMAL tokens: since the measures are discreet, they could not be any closer to chance level. On the other hand, it appears as though a quite substantial effect occurred under the SALIENT condition (a %-change of +17). However, nothing can be said with any degree of surety at these imbalanced levels.

As a very informal test of whether these figures reflect any reliable trend in the data, I randomly chose a contiguous pair of responses (responses 5 and 6) and tabulated them for the nine data sheets: of the 18 responses, 13 were to NORMAL stimuli, with 9 correct and 4 function errors; 5 were SALIENT responses, with 4 correct and 1 function error. Again there were no content errors. Comparing these results against the overall results, it appears there is a strong effect in the NORMAL condition for fifth and sixthness: the subjects did 18% better on NORMAL fifth and sixth sentences than they did overall. But for the SALIENT sentences, the difference amounts to a 1.5% decrease in accuracy. Comparing the fifth and sixth sentences to the final two of each randomization, there is yet another trend in evidence. The fifth and sixth sentences are +15% against the final two in the NORMAL condition, and -13% when SALIENT.

In other words, nothing of any certainty can be said about a possible effect the repetitions may have had. Any random pair of responses is likely to differ from the overall means in at least as marked a manner as the final pair does. However, in retrospect, it might have been wiser to go with only the cardinal 16 sentences, with no token repetitions, if only to spare both reader and writer from the previous two paragraphs.

3.2.5.1 Comparison of Means

The experiment and the data it produced were designed to facilitate three straightforward comparisons, two between-group, and one within the aphasic group: (1) the overall performance of the aphasic subjects vs. the overall performance of the normal subjects; (2) the SALIENT performance of the aphasic subjects vs. the SALIENT performance of the normal subjects; and (3) the performance of the aphasics for the NORMAL sentences vs. their performance for the SALIENT sentences. All three of these comparisons are between independent events. For the first two, this is true by definition, since no subject participated in both groups. For the within group test, the unique randomizations for each subject ensure independence: the probability of any given response is independent of any other response. On the surface, then, three independent *t*-tests should provide all the necessary statistics for the comparisons.

However, it is very difficult to support the standard assumptions of classical statistics like the *t*-test – gaussian distribution and homogeneous variance – with pathological subject pools.[20] Too many variables are involved and too little is known about them or their interactions. Consequently, aphasiologists often rely on nonparametrical techniques for avoiding the classical assumptions. Yet most of these tests have problems of their own. They tend to entail a higher probability of committing a Type II error. They still require the assumption of random sampling, "the most implausible assumption of typical psychological research" (Cotton [1973:168]), rendered even less plausible by work with brain-injured populations. And, more crucially for those of us uninitiated in higher mathematics, the justifications offered for Wilcoxon Rank Sum statistics, or Kruskal-Wallis chi^2 statistics either do not match our intuitions or are mysterious enough to preclude intuitions. Fortunately, there is a branch of statistics just under development that yields highly precise significance tests without dependence on gaussian distribution, homogeneity of variance, or even random sampling, and is founded on principles simple enough to satisfy mathematical neophytes: randomization tests.[21]

Randomization (or permutation) tests are methods of finding significance levels for any classical statistic. All the measures are assigned an index and the statistic of interest – a *t*-statistic, for instance – is calculated in the standard manner. Then the indices and measurements are permuted recurringly and a new *t*-statistic is calculated for each permutation. When all the permutations have been exhausted, the probability of the original *t*-statistic is determined by the proportion of permutations that produces *t*-statistics within its range. That is, the method substitutes brute "computational power for theoretic analysis" (Efron [1982:3]): if the experimental results are the product of random chance, the proportion of *t*-statistics close to the original *t* will be large, the probability will be high, and the significance low; if something other than chance produced the results – say, that the subjects were comprehending better under one condition than under another – the proportion and probability will be low, the significance high.

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[20] Hartley's test, suggested by Winer (1971:207) as a quite liberal test of variance homogeneity, produced an observed $F\mathrm{max}$ of 313.02, where $F\mathrm{max}_{(,5)}(2,8) = 4.43$, indicating very strongly that homogeneity is not supported in the data.

[21] See Edgington (1980:1-16), on which much of this discussion is based.

To carry out all the permutations and calculations necessary for the comparisons (48,620 permutations alone for each test), and to return the probability values, a FORTRAN program (RANDTTEST, Appendix F) was adapted from Edgington (1980:80). For all three comparisons, the significance levels were quite low: for test (1), the overall aphasic performance against the overall normal performance, $\alpha=0.00004$; for test (2), the SALIENT aphasic performance vs. the SALIENT normal performance, $\alpha=0.001$; for (3), the highly important test of SALIENT aphasic performance against NORMAL aphasic performance, $\alpha=0.01$. In short, all findings were very significant.

The comparison of overall means substantiates the findings of many recent studies on Broca comprehension deficiencies, most of which are outlined in Chapter 2: on a task crucially dependent on a brief, monosyllabic function word, Broca patients proved far less reliable than normal subjects of similar age, background, and gender.

The comparison of the two groups on a subset of items from the same test also substantiates these findings. However, the much lower probability indicates that the gap between the groups is not as substantial when the key function word is acoustically boosted.

The within-aphasic group comparison of NORMAL and SALIENT performance, on the other hand, resulted in new information. It has some support from earlier work – stress research, temporal manipulation research, and, incidentally, some function word research, as outlined in the previous chapter – but it is a much more explicit and interpretable finding. It suggests that a large part of the comprehension deficit Broca victims experience is peripheral to their specifically linguistic difficulties. A large portion of this deficit can be explained in processing terms.

4. Conclusion

Conclusions depend exclusively on premises. As with any study, the premises underwriting this one are varied and legion. Three in particular are essential, requiring both explicitness and justification.

1. **The syndrome under investigation – roughly correlating the anterior perisylvian cortical area and/or its subcortical projection with a cluster of largely productive symptoms – is legitimate.** The issue here is the opposition of Gordian language behaviour and the patterns excisable from that behaviour. Broca's aphasia is such a pattern, the most common, and it is very frequently consequent to Broca's area neural insult.

There are a number of arguments against the Wernicke-Lichtheim taxonomy, perpetuated by the BDAE and its subscribers. Most of these arguments (e.g., Schuell [1965]) are directed at the schema's supposed inappropriateness to clinical settings. But there are also a few which argue that it is similarly unsuited to meaningful linguistic generalizations of the type sought here, and sought by much of the research outlined in Chapter 2. One of the more immediately persuasive of these cases is presented by Schwartz (1984), who works from the position that aphasia is a dynamic phenomenon, misguidedly treated as static by the BDAE. But once she has discounted the Boston metric for unjustifiable rigidity, she has nothing to offer in its place. Overall severity, she points out, is an even less tenable measure; it cannot adequately account for qualitatively distinct disturbances. In short: it cannot account for patterns, and there are patterns.

Schwartz suggests aphasia might be best classified according to what she terms a "polytypic" procedure:

> some sort of multivariate space that charts simultaneous variation in [several underlying characteristics] as a function of, for example, site of lesion, age of patient, etiology, time since onset, etc. (16)

However, she pulls up far short of demonstrating how this procedure could be implemented, and there is very little surprise in this omission. The task she proposes is formidable enough in its synopsis, without having to fill in the vagueries of "some sort" and "etc". The Wernicke-Lichtheim schema, for all its tendencies to oversimplify and overlook, remains a highly valuable compass in the labyrinth of aphasia. It bears resolutely toward the most distinct syndromes.[22]

This is not to claim there are no significant dissimilarities among the individual cases grouped according to its classifications; given, among other things, the variability of responses measured in this study, that would be absurd. But although scatter and overlap among syndromes cannot be ignored, neither can it take centre court at the expense of important distinctions. There are taxonomic problems in all sciences, but there is always a point at which a set of assumptions is adopted and the haggling dies away. Biologists are still not resolved as to the classification of the platypus – some say it is a mammal, others, a reptile – yet both sides recognize its existence and find it particularly meaningful in the study of evolutionary theory.

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[22] Although this is slightly beside the point, the schema, at least in its BDAE manifestation, is a good deal less static than its detractors care to admit. It has built-in scaling methods for such important variables as severity, and allows all manner of mixed categories; it does not stop at a Wernicke's/Broca's dichotomy. And many therapists — who perhaps have a more urgent need to recognize patterns than do linguists — find it very useful. A recent survey answered by 121 British therapists (Beele [1984]) found that the BDAE was their preferred test, and virtually all the therapists I consulted in the course of this study employed it, if only in select subsections.

Broca's aphasia is a recognizable and meaningful symptom cluster, the most consistent such cluster to emerge from the knot of neural insult language disorders.

2. **Investigating Broca's aphasia can provide useful information about Broca's area, and the function it subserves in normal language use.** The first clause of Linebarger *et al.* (1983) is: "the phenomena of aphasia are of psycholinguistic interest primarily insofar as they shed light on normal language processing". *Primarily* is overstating the case somewhat, since many psycholinguists are concerned with aphasic phenomena for what they reveal about aphasia. But the point should be well taken – perturbations in a system are unique and valuable clues as to the processes underlying its normal operation. And even those researchers investigating aphasia *qua* aphasia are ultimately interested in ways to reverse its debilitating

effects back toward normal processing levels.

The issue is another familiar one – whether or not pathological behaviour has any bearing on the behaviour of similar individuals who do not share the pathology. On some levels, the question must be resolved in the negative; on others, a qualified affirmative can be advanced. Again there are premise dependencies. Take, for example, the case of two relatively parallel individuals, matched in at least the major respects known to influence language. One of them has a CVA, a left anterior CVA, and begins to exhibit marked linguistic differences from the other (and from his own premorbid state). It is reasonable to assume that these symptoms are the product of some interaction between the normal language system, still on display in the individual fortunate to have escaped a CVA, and this new factor, the lesion. An investigation of the pathology is an investigation of this interaction; necessarily, it considers the normal system and the damaged one, to better profit than considering either in isolation.

There are a number of possibilities for the interaction. It might be that Broca's area is primarily responsible for monitoring digestion and, since digestion is such an essential biological process, evolution has arranged for a back up system to kick in. This emergency component is supported by cortex normally devoted to language, a biologically expendable function. The result is that language processing is incidentally disrupted and digestion goes on unabated. The scenario is conceivable, but when other postulates are considered – such as the thoroughly researched cross-species role the thalamus plays in digestion – its plausibility plummets rather quickly.

On the other hand, it might be that Broca's area has some principal responsibility to the language faculty – it might be instrumental in signal processing – and its destruction or impairment affects language behaviour directly. I don't mean to imply that there are only two possibilities here, digestion or parsing. But there are two classes of possibilities, incidental disruption and direct disruption, with the latter as the most likely. And the plausibility of direct involvement increases with the introduction of other postulates, such as the simulated aphasia effected by Penfield and Roberts' (1959) probing in Broca's area, and cerebral blood flow findings, (e.g., Ingvar and Schwartz [1974])

However, much of the evidence in pathology is constrained to the negative. That is, it supports confident statements about what functions the defective region does not serve, but only cursory speculation about the role it in fact plays. So, if our CVA victim breaks into fluent swearing, it must be concluded that he knows how to swear and that all the necessary peripheral mechanisms for implementing that knowledge are relatively intact. A proposition follows with some certainty that the damaged area does not significantly subserve cursing, either in its steady or its engaged state. If this behaviour is observed in a majority of the cases with the syndrome (at least those who swore premorbidly), then it would be perverse to claim that either aphasics or nonaphasics have cursing stored or activated in Broca's region.

To exhume the competence / performance distinction, cerebral pathologies can inform about the presence of knowledge, but not its absence. If a class of brain-injured patients can no longer understand the subtle meaning cued by placement of *the* – or, more precisely, if they can no longer demonstrate such understanding relevant to a given task – the evidence is insufficient to presume that the word or the meaning is no longer represented cognitively. The elegant organization of the brain is too complex and too poorly understood to support claims about knowledge or abilties when there is no overt evidence, except in the most general and speculative ways; *argumentum ad ignorantium* is no less a fallacy in disciplines where the level of ignorance is high. On the other hand, if the same class demonstrates comprehension of *dog*, their knowledge of the word and the processing necessary to apply that knowledge can be said to be functional. It would be bizarre in such a situation to claim that speakers, with or without the syndrome, store *dog* in the damaged area, or that the area is crucial to processing it. Precisely the same must be true for *the*. If a class of pathological individuals demonstrates comprehension of the meaning cued by *the* placement, say, the class of Broca's apasics, it would be just as bizarre to claim that the lesion site is instrumental in storing or processing *the*.

3. **Experimentation can uncover useful information about aphasic syndromes and, indirectly, about the brains and language faculties of people without the syndrome.** Aside from some of the more mystic "New Physicists", it is well accepted that experimentation is a valuable method of enquiry. But it has rather strict limits, and one of the strictest is that experimental conditions are, by definition, artificial. Mesons may not occur in nature, because the conditions of a particle accelerator may not occur in nature. However, the fact that mesons are torn away from larger particles consistently in cyclotrons indicates that, in the right conditions, they *can* occur in nature. Working with other postulates, it can be inferred that they *may* occur in nature. The more robust those postulates, the more they converge on one set of answers, the greater the probability that they *do* occur in nature.

Returning to aphasia, demonstration of competence for *dog* or *the* in an unnatural linguistic setting does not, *a priori*, demonstrate that the competence can be activated in natural settings. With *dog* the situation is not especially distressing, since naturalistic observation is likely to confirm or augment experimentation; with *the*, the problem becomes critical. It is extremely unlikely (1) that sentences whose disambiguation hinges on the presence or location of *the* will occur nonexperimentally, (2) that the natural context will not suffice on its own to disambiguate such sentences, should they occur, and (3) that the event would be apparent to an observer. The only way to gather postulates about such knowledge is experimentally. But if a demonstration of competence can be engineered in artificial situations, it is reasonable to conclude that, with the right conditions, comprehension *can* occur outside the experiment. More to the point, since the primary conditon, impaired cognition, is not artificial, there has been a demonstration that comprehension *does* occur in the pathological brain.

Once again, failure to demonstrate *the* competence, whatever that might be, does not signal its absence. In my paradigm, there are at least three indispensible abilities, above and beyond comprehension of *the*, which are required for reliable trial performance: (1) the subject must be able to process the sentence; (2) he must be able to assess the pictures; and (3) he must be able to effect an explicit connection between the two presentation modes. Fortunately, aphasics become quite practiced at these skills, and Broca victims are generally very good at picture-pointing tasks (Seron and Deloche [1981]); nevertheless, a breakdown at any one of these levels, or any one of possibly scores more, can interrupt the process and result in failure. But success is meaningful. It entails all three abilities and it entails comprehension of the sentential reading cued by *the*. And better success under one condition than the other entails that something peculiar to the condition is instrumental in the comprehension.

Taking the in-context complex of these three assumptions – essentially, that my experiment has generated useful information for aphasiology in specific and psycholinguistics in general – it is perhaps time to examine that information.

The most immediate result of the experiment is the demonstration of a comprehension deficit for some function words in Broca's syndrome. This amounts to a successful replication of the findings discovered by the study taken as a prototype for this thesis, Heilman and Scholes (1976). They found that Broca patients had impaired comprehension that implicated processing of *the*, and of the argument structure cued by its location. The massively significant (α=0.00004) difference between the aphasics and controls showed this deficit to be very distinct in the group I studied. Moreover, Heilman and Scholes found their Broca pool's mean proportion of syntax (function) errors to be 0.42, slightly below chance level. My replication found the mean function error in the condition that matched Heilman and Scholes' presentation (i.e., NORMAL) to be 0.46: a proportion of 0.58, insignificant from chance at $p<0.1$. (The mild discrepancy may be due to Heilman and Scholes statistical procedure of correcting for lexical errors in their calculations of the syntax proportion, a procedure I did not adopt.) This finding is also consonant with several other function word experiments, such as Goodenough *et al.* (1977).

However, the introduction to Heilman and Scholes' paradigm of a second, SALIENT, condition licensed the detection of some differences the original was not equipped to find. The Broca patients I tested proved significantly more reliable (α=0.01) on the SALIENT sentences than on the NORMAL ones. Since the differences between these two conditions were acoustic and localized to only the key functors, this result indicates that part of their failure in the NORMAL condition (and, by extension, part of the Heilman and Scholes pool's failure) was the product of defective signal processing. Further, while the difference between normals and aphasics for SALIENT sentences remained statistically significant at a very low α-level (0.001), the gap between these groups reduced as a function of increased salience. (A NORMAL normal vs. NORMAL aphasic *t*-test proved significant at α=0.00002.)

It is at this point that the results of this study become most interesting: the uncovering of a significant SALIENT effect has implications for competence theories of the sort entertained by Heilman and Scholes, and promulgated by authors like Zurif and Kean. In particular, it compromises the claim made by Scholes (1978) for his work with Heilman:

> the findings ... support the view
> that there are autonomous lexical

> and syntactic components in the neurolinguistic model of language ... and that these components can be localized as posterior and anterior respectively. (183)

And it compromises the strong form of any similar hypothesis, such as Bradley, Garrett and Zurif's (1980:284) suggestion that Broca function word peculiarities follow from the "loss of specifically syntactically supported language processes", or Kean's early phonological word claims. While most of these theories prefer to take more politic routes than the fragile formulations advocated by Popper, and consequently are not easily falsified, this finding at least has a tempering effect on their plausibility. The reason it compromises the strong form of any functor loss theory – and mitigates the weaker forms – is, quite simply, that it shows the functors are not lost. They might be obscured, difficult to recover from the signal or the mind, but they are not missing outright.

The saliency effect, of course, is not altogether unexpected. It is in accord with several of the functor research papers reviewed in the second chapter, such as Mack (1981), who found quite good comprehension using multisyllabic, stressed function words. It also follows rather naturally upon many of the acoustic manipulation experiments reviewed earlier, such as Goodglass *et al.*'s (1967) finding that Broca victims were significantly better at repetition tasks as a function of stress placement. But the result is more specific and interpretable than these earlier findings, due to the coupling of an elegant design and very precise stimulus controls. It does not require extrapolation from statistical insignificance, as Mack does, and it depends on comprehension, not simple retention, as does the Goodglass *et al*. paradigm.

Unfortunately, the study cannot go much further on its own. It quite clearly identifies an acoustic dependency in Broca comprehension difficulties. But it does not support more specific claims on the nature of that dependency. It does not speak directly to the impaired mechanisms behind the acoustic problems. If there had been more conditions in the experiment – perhaps independent manipulations of duration, intensity and f_0, or substitution of a 200 msec gap or noise burst for the functor – then the study might have provided answers keyed more directly to particular aspects of signal processing, and investigation of this sort is certainly suggested by the present study. But nothing more specific can be said at this point than: (1) there is a performance aspect to the functor comprehension deficit in Broca's syndrome; (2) the competence proposals for the deficit are unsupported, possibly wrong; and (3) Broca's area is at least partially involved in low level signal processing.

Still, there is always speculation, and my feeling is that a major culprit in the Broca functor difficulties is defective short term memory (STM). There are several reasons supporting this impression. First, many of the papers reviewed in Chapter 2 impinged somewhat on STM. For instance, the very first paper discussed (Efron [1963]) was based on a sequencing task which discovered that Broca patients required significantly more time than normals ($p<0.001$) and other aphasics ($p<0.01$) to identify the order of frequency pulses, and Efron's finding has been replicated by Edwards and Auger (1965) and Ebbin and Edwards (1967). Laughlin *et al.* (1979) administered a temporally controlled version of Melodic Intonation Therapy and found that the more time the subjects were given, the better they recalled the intoned phrase (significant beyond the $\alpha=0.001$ mark). The just discussed Goodglass *et al.* task also involved recall, which varied significantly as a function of stress patterns. And Liles and Brookshire (1975) found that interpolated silences only facilitated comprehension of their TT requests when there was very little memory strain; the later in the sentence, the less the gaps helped.

Second, there is a quite robust series of studies which directly investigates STM loss in aphasia. All of them have found substantial memory deficits, and several experiments have found more profound losses in Broca's syndrome than in other aphasias. Another study headed by Goodglass and reported in Chapter 2, is instrumental here as well. Goodglass, Gleason and Hyde (1970) also gave their subjects a comprehension task with a sequencing component. They orally presented simple noun sequences (of between two and six items), and the subjects were required to reconstruct the sequence by selecting the named items from an array of eight pictures. The best of their four aphasic groups, the anomics, scored below the worst of their normals, six-year old children, and the "Broca's aphasics had by far the poorest score" (595), significant at $\alpha=0.004$.

Swinney and Taylor (1971) also gave their (predominantly expressive) aphasic subjects a sequencing task: tachistoscopic presentation of two, four, and six item digit lists which the subject attempted to reconstruct, uncovering an error rate 20 times greater than the controls and significantly greater response latencies. Albert (1976) found the same results with his very straightforward sequencing task: "in this order, point to the A, the B, the C, and the D", where each upper case letter stands for the name of some commonplace item (a comb, a pen, etc.) on display in front of the subject. The aphasics proved to be significantly impaired ($p<0.001$) in both accuracy and sequencing with respect to normals and brain-injured nonaphasics.

Recall that Efron (1963:418) suggested his expressive subjects' performance might be the product of "a primary deficit in temporal analysis"; that is, a sequencing impairment, and Goodglass *et al.* (1970), Swinney and Taylor (1971), and Albert (1976) all support that position. But they also support, as does Efron (1963), another deficit – of defective STM – and several other studies have explored this possibility by way of simple recognition tasks, without sequence dependencies. They indicate the problem may not be in analysis, but in retention.

Cermak and Moreines (1976) used a recognition task with a predominantly anterior (exclusively anterior for one subtest) aphasic subject pool they were using as part of a general memory investigation. They tested for identification of repeated letters, repeated words, same category words, and rhyming words, and they found that the greater the number of intervening items, the worse these subjects did. They were significantly ($p<0.05$) worse on all but one task than normals, right hemisphere nonaphasics, Korsakoff patients, and nonKorsakoff alcoholics; the only group that performed worse, on one task, was the Korsakoff pool, victims of a syndrome identified with memory deficiencies. Cermak and Moreines were understandably quite surprised at this outcome:

> The aphasic patients seemed to be perfectly capable of detecting repetitions when the target items were adjacent to one another, and even when one or two items intervened, but were worse even than the Korsakoff patients as soon as their running memory load became taxed beyond this point. Apparently, the aphasics' working memory is actually inferior to patients whose primary diagnosis is amnesic. (26)

Cermak's curiosity was piqued by this outcome, and he continued the investigation in two subsequent studies. The most recent – Cermak, Stiassny, and Uhly (1984) – is more or less a straightforward replication, which again found the aphasic pool (this time exclusively Broca patients, n=8) to perform worse than the Korsakoff's and the other controls, again with a between group significance level of $\alpha=0.05$. The second study is a little more interesting. Cermak and Tarlow (1978) gave their subjects both linguistic and nonlinguistic stimuli. With the linguistic stimuli, oral and visual word lists of between 7 and 14 items from which repetitions were to be identified, their exclusively anterior pool again did significantly worse than all other groups, including Korsakoffs ($p<0.05$). But for the two tasks with nonlinguistic stimuli (7 - 14 item displays of pictures or randomly generated shapes), they were not significantly differentiated from the other pools.[23]

Cermak's work is particularly important here, for the success with which it isolates the memory deficit. On the one hand, it locates a problem distinct from (and possibly responsible for) a sequencing deficiency. On the other hand, it separates off problems of analysis, pattern matching, and (very) immediate memory. The subjects of Cermak and Moreines (1976) and Cermak *et al.* (1984) were clearly successful at analyzing acoustic signals, at briefly storing them, and at matching the memory with a repetition. But their performance degraded dramatically as soon as the STM load went beyond two items. Moreover, Cermak and Tarlow

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[23] For other memory deficit studies with aphasics, cf. Shallice and Warrington (1970), Heilman *et al.* (1976), Locke and Deck (1978), Caramazza *et al.* (1978), Rothi and Hutchinson (1981) Gordon (1983), Ostergaard and Meudell (1984), and Ostergaard (1984).

(1978) isolate the deficit as an auditory one (since reading involves auditory reconstruction), or a linguistic one, distinct from other memory mechanisms.

It is also instructive here to keep in mind both the spectrograms of Appendix C and the HWIM system under research at Harvard. HWIM works by way of spectrographic analysis and scans for reliable acoustic clumps from which to derive hypotheses about meaning. It, too, has trouble with the acoustic clumps that give Broca patients so much difficulty, small function words. The NORMAL spectrograms are a graphic display of why these words pose the problems they do: they are not very distinct. If it is considered that the spectrograms are very good ones, the product of a detailed digital analysis that might not be available to cognitive analyzers, and that the speaker was a professional, enunciating clearly and carefully, the difficulties unstressed functors can cause become more apparent still. And, of course, a spectrogram is a memory.

In fact, a Sona-Graph could be accurately called a short term memory, or at least its central mechanism could be so designated. It records very limited (about 2 sec) acoustic sequences, and as soon as its tolerance is reached the incoming signal begins to copy over the original memory, obliterating it. There are some obvious constraints on this analogy – principally, that the Sona-Graph's capacity is defined wholly in temporal terms, while STM works in units, and the interstimulus periods are unrecorded – but it is a useful one nonetheless. If, as some psychoacoustic research implies, the initial "auditory image ... present[s] itself in the form of a neural spectrogram ... [which] is continuously being replaced by new information" (Borden and Harris [1980:203]), then the task of analyzing and storing minor acoustic clumps is revealed as quite remarkable. With a defective STM, of about two items in capacity, such clumps could very well be deemed expendable. But the SALIENT versions of those minor acoustic clumps represent the same information much more distinctly, and might well make as much of an impression on such a memory as they do on the spectrograms.

In brief, what might be going on with the SALIENT stimuli is a von Restorff effect. A commonplace in memory studies is that any prominent item is a sequence in easier to retain, recall, and manipulate than the same item without emphasis: a long, loud, distinct function word, in a normally reduced position, violating expected intonation patterns, can safely be labelled "prominent".

There is also a less scientific, informal reason to believe my subjects did less well on NORMAL than on SALIENT sentences because of memory restrictions. A number of their final answers were clearly manifestations of a memory intrusion, since they would select a picture, then change their minds and select another. These reversals were not recorded, but it is my impression that the majority of them were corrections of function errors, and that the majority occurred with SALIENT stimuli, though certainly this process happened in both conditions, and at least one SALIENT response reversal was from correct to function error. Some of the normal subjects also exhibited memory interference, or more correctly, memory monitoring, since all of their reversals were corrections of function errors. Several of the aphasics also occasionally echoed the last phrase or so of the stimulus sentence before making their final decision. One of the most successful subjects (JC) followed this strategy for 17 of the 18 sentences: his three function errors were preceded by an inaccurate echo, and his content error occurred on the one sentence for which he neglected to follow the strategy.

Heilman and Scholes do not mention such behaviour in their paper, but it is interesting to note that one suggestion they offer for the performance pattern they uncovered in Broca patients is that the subjects "could only recall the major lexical items" (63), and the next paper they collaborated on was an explicit investigation of aphasic memory (Heilman *et al*. [1976]). This characterization in fact might be the most accurate appraisal offered for the comprehension difficulties of Broca patients. If they do process many sentences as content word anagrams, structureless and somewhat uncategorized – as a good many studies, including this one, suggest – that anagram does not necessarily have to follow from syntactic incapacity, or lexical loss, or phonological word processing restrictions. It might be because that is all they can remember.

As above, there were not enough controls in my study to support this speculation exclusively. The better SALIENT performance could also be explained as the result of poor acoustic resolution, or some specific subcomponent of this ability, such as temporal resolution; certainly there is enough other evidence that duration is a factor in aphasic comprehension. Similarly, it might be ascribed to disturbed attention span. It is well known that boosting duration and amplitude also boosts the hearer's attention levels. It might be that the saliency effect can be explained by a resource allocation model, of the sort discussed in Kahneman (1973) and investigated in childhood aphasia by Campbell and McNeil (in press). That is, the answer may lie in a more specific exploration of the relationships between the signal processing ends of STM, attention, and resource allocation; they are perhaps different terms for, and perspectives on, the same cognitive procedures.

But the saliency effect cannot, at least without a great deal of unapparent ingenuity, be explained as the manifestation of some competence deficit. Knowledge is either present or absent: if it is differentially accessed as a function of external factors, then it is present and there is something amiss with the access system; if it is absent, it cannot be accessed. My subjects clearly had some access to *the* and some idea of how to parse the sentence readings cued by its location, both of which were influenced by acoustic manipulations. In Sasanuma *et al.*'s (1973:72) words, this result suggests "the presence of a certain kind of 'noise' or defect in the auditory system ... at a stage preceding speech comprehension". Defective memory processes seem the most likely generators of this noise.

It may seem that the research undertaken in this thesis has the principal objective of dissociating two fundamental aspects of Broca's complex that much of recent psycholinguistics has sought to conjoin. It has long been known that a central component of Broca's syndrome is agrammatic speech, but only in the last decade or so has the possibility that it may also entail agrammatic comprehension been explored with any thoroughness. Psycholinguists working on this problem have been buoyed by the prospect that production and comprehension might parallel one another, and consequently help to validate the modular view of language offered by most formal linguists. Taking a grammatical competence model like the traditional Standard Theory transformational grammar, or like any one of its numerous offspring, and selectively incapacitating one component should result in a uniform output / input deficit. If syntax, or semantics, or phonology, or the lexicon, were disrupted, then both understanding and expression would be coincidentally distorted. Therefore, an aphasia which manifested parallel disturbances would be a compelling argument for the psychological reality of language modularity.

Broca's aphasia was the most likely place to start — since it is the most common form of aphasia, it is relatively well defined, and its victims are more manageable subjects than those of syndromes like global or Wernicke's aphasia. But as more and more investigations were launched into the "syntactic deficit", it looked less and less that the loss was uniform. Sometimes the Broca subjects behaved in perfect accord with the theory; other times, they violated its predictions. One paper claims to have uncovered the component and the disruption that effects agrammatism (Heilman and Scholes [1976]); another produces counter evidence (Mack [1981]).

But there are consistencies in the results that few noticed or commented on. For one thing, the Broca production problems are not uniformly agrammatic either. Broca patients occasionally speak in telegramese, but their speech is often very different than the telegrammatic outputs observable in pidgins or in children undergoing acquisition. Broca patients frequently include function words, sometimes repeating them several times before hitting a noun or verb. It is this behaviour that scuttled the old economy of effort hypothesis (Goodglass [1968]). Second, this irregularity is present in their comprehension as well. Sometimes they understand a function word. Other times, they do not. And third, there is a pattern to their comprehension of functors that is at least as systematic as their output pattern. Quite consistently, when semantic and pragmatic variables are constrained they comprehend multisyllabic or compound function words, while failing to comprehend short, monosyllabic, unstressed ones.

Is then the attempt to uncover a single mechanism responsible for parallel production and comprehension deficits ultimately a misguided one? No, that doesn't follow, though it may be that the attempt to explain such a loss in neat competence terms will prove misguided. It would not, for instance, take a great deal of imagination to draft a model of language processing that involves a shared memory buffer for production and comprehension, or that uses a spectral analyzer to monitor output as well as input. But, since both production and comprehension have their own irregularities in Broca's syndrome, in addition to the parallels, the sources of variation should receive at least as much attention as theoretical sources of similarity. It is the patients, not the linguists, we should be trying to help.

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APPENDIX A:*Additional Aphasic Data*

entioned in Chapter 3, this study also gathered data from several aphasic patients who were excluded from the primary analysis. Although there are very good reasons for these exclusions, it would be inappropriate to discard the information altogether. It would be unfair to the patients, who kindly sat through the experiment in the expectation that their contributions would be of some scientific benefit, and it would be unfair to the readers of this study, who are aware that aphasiological data are far too precious to leave mouldering in a file jacket.

But it is important to keep in mind that the following reports and commentary are addenda only, and do not constitute a further refinement of the information already presented. It is not, for instance, appropriate to think of the gentleman whose diagnosis was distinctly nonBroca as a control for another syndrome. Neither would it be legitimate to suggest that the two non-native English speakers show the findings reported above to generalize from native to non-native speakers. The information these subjects provide is suggestive, nothing more, and must be interpreted cautiously.

The five subjects not included in the analyses reported above are described in Tables A.1 and A.2; the former contains general descriptions, like those of Table 3.1, while the latter parallels Table 3.2. These subjects were recruited through the good offices of one Edmonton hospital

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Table A.1

GENERAL DESCRIPTIONS, ADDITIONAL SUBJECTS

Subjects	Age	Gender	Education (Years)	Visual Acuity	Auditory Acuity
MK	73	male	n/a	good	fair
GP	66	female	gymnasium	good	fair
LS	58	male	19 (M.D.)	good	good
JC	58	male	n/a	good	good
EH	48	male	n/a	good	good

NOTES:

GP is a native German speaker who as been speaking Canadian English for over 30 years.

MK is a native Ukranian speaker who has been speaking Canadian English for over 30 years.

---

and several Vancouver hospitals. All were victims of left CVAs; two had anterior lesions, but more specific localization was unavailable for the other three. All but one (MK) were outpatients, and MK was as severely hampered by hemiplegia as by his linguistic problems and was not scheduled for release. The mean age for this group was 52.6 years (s^2=25.7), and their mean postonset time was 16 months (s^2=12.77). All had good vision, though again several wore corrective lenses, and relatively good hearing. All were right-handed, but there was a note in LS's file to the effect that he had been "left-handed as a child". If this means that he was trained into right-handedness, as was not uncommon as recently as two decades ago, this could mean that a substantial portion of his language development did not follow the normal lateralization pattern of

Table A.2

APHASIA SPECIFIC DESCRIPTIONS, ADDITIONAL SUBJECTS

Subjects	CVA Postonset	Apraxia	Dysarthria	Hemiplegia / Hemiparesis	Output / Comprehension Severity	Session Mood / Alertness
MK	29 months	severe	mild	R, severe	severe / moderate	friendly / tired
GP	3 months	moderate	mild	R, mild	moderate / mild	friendly / alert
JC	28 months	moderate	none	none	moderate / moderate	friendly / alert
LS	3 months	moderate	mild	mild	mild / mild	friendly / alert
EH	17 months	"queried"	mild	R, mild	moderate / moderate	eager / alert

NOTES:

LS was diagnosed as a nonaphasic apraxic.

EH was diagnosed as "Mixed, Non-Fluent".

JC was appraised as a Wernicke's aphasic.

MK and GP had left anterior lesions, but there was no lesion information on LS, EH, or JC more specific than "left".

right-handers, and it might help explain the mildness of his aphasia. His score on the BDAE comprehension subtests was 100%, and his therapist had diagnosed him as exclusively apraxic, without language deficits (though his performance on the experimental task was far below normal levels and reveals some degree of language impairment). He was also quite anxious not to appear impaired. He asked how he did on the task and explained away his errors by saying the bird in the pictures should have been called a parrot (two of his errors occurred on the bird sentences). All were diagnosed as "Non-Fluent", with the exception of JC. When I saw him he was quite fluent and in conversation only suffered minor word-finding difficulties. His regular therapist was unavailable for consultation, but my hospital liaison assumed his lesion was posterior and that he was a largely recovered Wernicke's case.

The responses for this group are displayed in Table A.3. The most curious result in Table A.3 is the performance of MK, especially as it relates to the performance of FD, reported earlier. These men are very similar in several respects: age, institutionalization, levels of severity, and in performance on the experiment. This would not be surprising (AM and DS are also very similar on all these metrics) except for the pattern of that performance. Both men did *worse* than chance level on the NORMAL stimuli, and very close to chance on the SALIENT stimuli. Since there is no reason to believe that there is

Table A.3

MEAN RESPONSES, ADDITIONAL SUBJECTS

Subject	Condition	Correct	Funct Error(s)	Cont Error(s)
MK	NORMAL	2	5	1
	SALIENT	4	4	0
GP	NORMAL	2	6	0
	SALIENT	6	2	0
LS	NORMAL	5	3	0
	SALIENT	7	1	0
JC	NORMAL	3	5	0
	SALIENT	6	2	0
EH	NORMAL	3	5	0
	SALIENT	6	2	0
$\bar{x}$	OVERALL	9.2	6.6	0.2
	NORMAL	3.4	4.2	0.2
	SALIENT	5.8	2.2	0
s^2	OVERALL	2.168	1.816	0.447
	NORMAL	1.44	0.836	0.447
	SALIENT	1.095	1.095	0

something about the former that would lead these patients to systematically misinterpret the sentences in the function error direction, and nothing about the latter that should improve misinterpretation to guess level, this parallel must be a coincidence. But it is a curious one nonetheless.

Another unpredicted outcome is the performance of JC. If he is indeed a Wernicke victim, it is somewhat unexpected that he would not pattern a little closer to the Wernicke pool investigated by Heilman and Scholes, but he made no content errors at all, he was close to chance for the NORMAL stimuli, and he evidenced a SALIENT effect. My suspicion is that he might be a conduction, or transcortical, aphasic. Though the therapist who was present clearly ventured a more educated guess than mine, she did not have the results of any examination to go by; I tested JC on the comprehension screen and on the experiment proper, and conversed with him for some time. If he is better diagnosed as a Conduction, then his performance is in consonance with the Heilman and Scholes results; if he is a Wernicke patient, then his performance is somewhat anomalous, though anomalies are not unusual in aphasiology.

The three remaining patients patterned pretty much as those discussed in the primary analysis. LS and EH were close to chance level for NORMAL stimuli, and better than chance for SALIENT stimuli. GP was also better than chance with the SALIENT stimuli, though she was worse than chance for the NORMAL sentences. On the average, this group performed slightly less reliably than the primary group, but the pattern is similar, and there is a marked salience effect.

(Post hoc randomized *t*-tests were conducted on these data, with the following results, at $\alpha=0.05$: the NORMAL results of this group did not differ from chance level; this group did not differ in overall scores from the previously analyzed group; they did not differ on the SALIENT stimuli; however, they did significantly worse on the NORMAL stimuli than the primary group.)

APPENDIX B: *Consent letter / release form*

Dear (Mr. /Mrs. appropriate name here):

I am studying language at the University of Alberta. I would like to include you in my study.

The study consists of one session, much like the session you have with your speech therapist.

I will keep your comfort and welfare in mind during all sessions. You may stop any time if you wish.

Your name and other personal information will not be reported.

If you wish to help me in my study, please put a check mark in the space on the bottom of this form, and please sign the form.

Thank you very much for your time, and a special thanks if you have decided to help.

-- Randy Harris

I have read this letter and wish to help the study: ______
Signature:____________________
[I countersigned here, with the date.]

APPENDIX C: *Alligator programs for assembling and randomizing the stimulus sentences*

```
RPLAY
C Assembles stimulus sentences in 2 conditions, from sets
C of 3 segments (condition 1 = HEADx + THEx + TAILx;
C condition 2 = HEADx + PTHEx + TAILx).  HEADNTAIL is a
C signal file containing sentence HEADs and sentence TAILs.
C THES is a signal file containing THEs and PTHEs.  RLIST
C is a text file containing the randomized numbers 1 - 16
DATA NWAIT 10
DATA PAUSE 20
DATA HEAD*8
DATA THE*8
DATA TAIL*8
DATA I
SET FREQ=10
GET HEADNTAIL
GET THES *IN
SOURCE RLIST
LABEL 1
CL WA
READ *SOU &I
IF &I GT 8 GOTO 2
ENCODE &HEAD HEAD &I
ENCODE &THE THE &I
ENCODE &TAIL TAIL&I
LOAD &HEAD
LOAD *IN &THE
LOAD &TAIL
Q A &HEAD &THE &TAIL
P
WAIT NWAIT SEC
GOTO 1
LABEL 2
SUB &I 8
ENCODE &HEAD HEAD &I
ENCODE &THE PTHE &I
ENCODE &TAIL TAIL &I
LOAD &HEAD
LOAD *IN &THE
LOAD &TAIL
Q &HEAD
P
WAIT &PAUSE MSEC
Q &THE
P
WAIT &PAUSE MSEC
Q &TAIL
P
WAIT &NWAIT SEC
GOTO 1
END
```

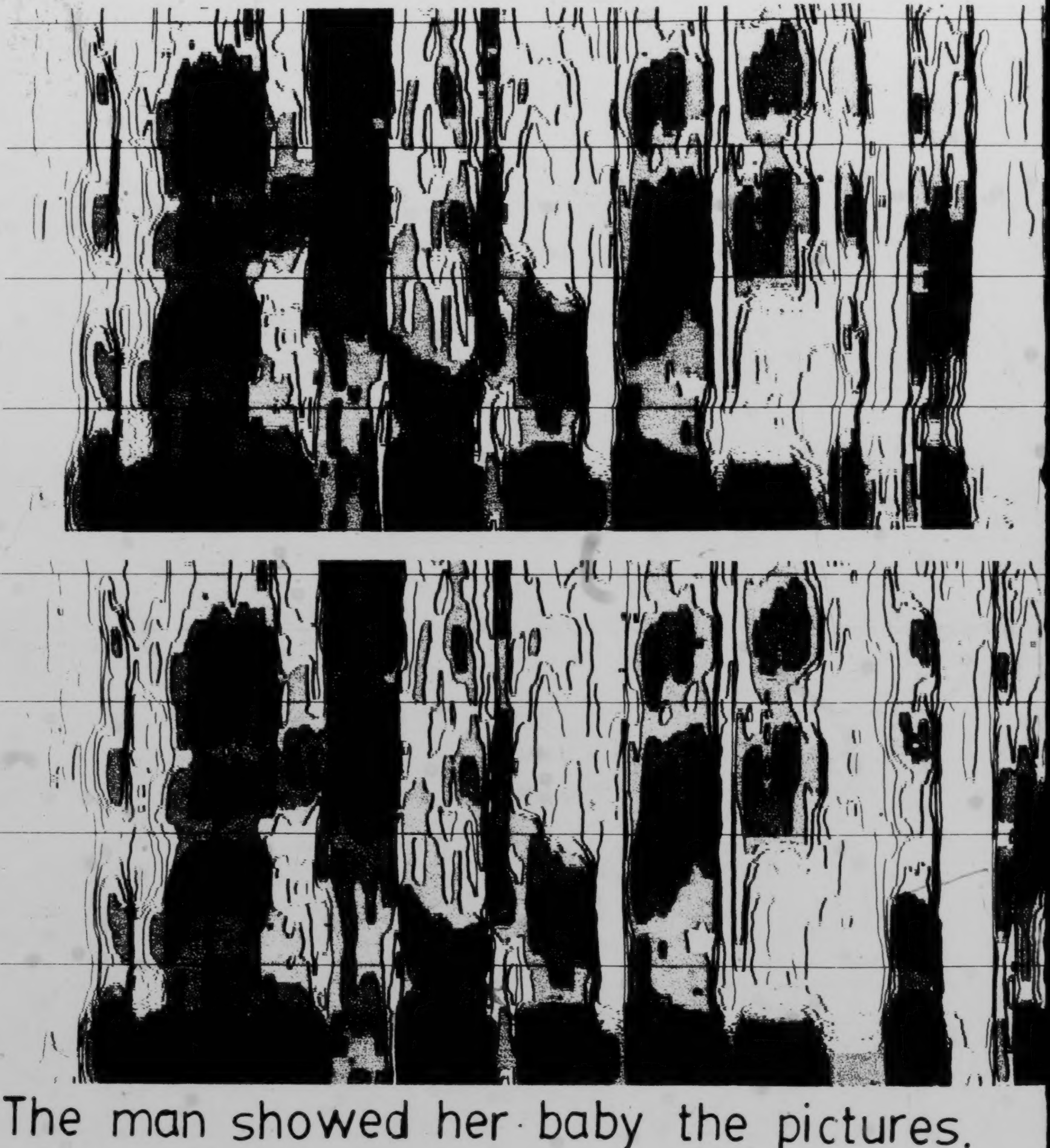
```
RAND
C A PROGRAM TO GENERATE A
C RANDOM SEQUENCE OF 16 NUMBERS,
C USED BY RPLAY.  SEED IS A 5-DIGIT
C NUMBER TAKEN FROM A RANDOM NUMBER
C TABLE, ENTERED ON THE TERMINAL
DATA NUM(16) 1 2 3 4 5 6 -
7 8 9 10 11 12 13 14 15 16
DATA SEED
PRINT SEED?
READ *TTY &SEED
EMPTY RLIST
SINK RLIST
RANDOMIZE *SINK &SEED 1 &NUM
END
```

```
MEASDA
C A PROGRAM TO MEASURE THE
C DURATIONS (MSEC) & AMPLITUDES
C (PEAK, AREA & AVERAGE RMS) OF
C THEX & PTHEX; X = 1 - 8.
C SEGNAMES IS A LIST OF NAMES
C FOR THE MEASURED SEGMENTS.
DATA X
DATA Y
DATA SEG*6
SET FREQ=10
SOURCE D0:SEGNAMES
GET D0:THES
LABEL 1
CLEAR WA
READ *SOURCE &SEG
LOAD &SEG
MEASURE DURATION &X &Y MSEC
PRINT &SEG
PRINT DURATION = &X &Y
MEASURE PEAK &X &Y
PRINT PEAK = &X &Y
MEASURE AREA &X &Y
IFERROR 50 PRINT 50
PRINT AREA = &X &Y
MEASURE AVER &X &Y
PRINT AVERAGE = &X &Y
GOTO 1
END
```

```
MEASP
C A PROGRAM FOR MEASURING THE LENGTH OF PITCH PERIODS
C ON AN ACTIVE SIGNAL.  FILE & SEGMENT ARE READ FROM
C TERMINAL, SEGMENT STORED IN TEMP.  THE PROGRAM
C ADJUSTS CURSOR POSITIONS BY THE DURATION OF THE
C PREVIOUS PERIOD & PRINTS THE NUMBER OF DATA POINTS
C FOR THAT PERIOD.
CLEAR WA
RELEASE
EMPTY D0:TEMP
SET NTH=1
RELEASE
DATA A 0
DATA B 0
DATA F
DATA G
DATA N
DATA FILE*6
DATA VOW*8
PRINT ENTER FILE, SEG
READ *TTY &FILE &VOW
LABEL 1
GET D1:&FILE
ADD &N 1
CLEAR WA
LQ &VOW
ADJUST &VOW ENTRY &A &B
RELEASE
EMPTY D0:TEMP
GET D0:TEMP
SAVE &VOW
CLEAR WA
LOAD &VOW
LABEL 3
QUEUE &VOW
EDIT
LOCK
WAIT 20 SEC
WAIT 20 SEC
LOCK
TRUNCATE
RETURN
PLAY 30
WAIT 15 SEC
IF SS:0 EQ 1 GOTO 3
MEASURE DURATION &F &G PTS
PRINT PERIOD &N &G PTS
ADD &A &F
ADD &B &G
LABEL 5
IF &B LT &G ADD &A 1
GOTO 1
END
```

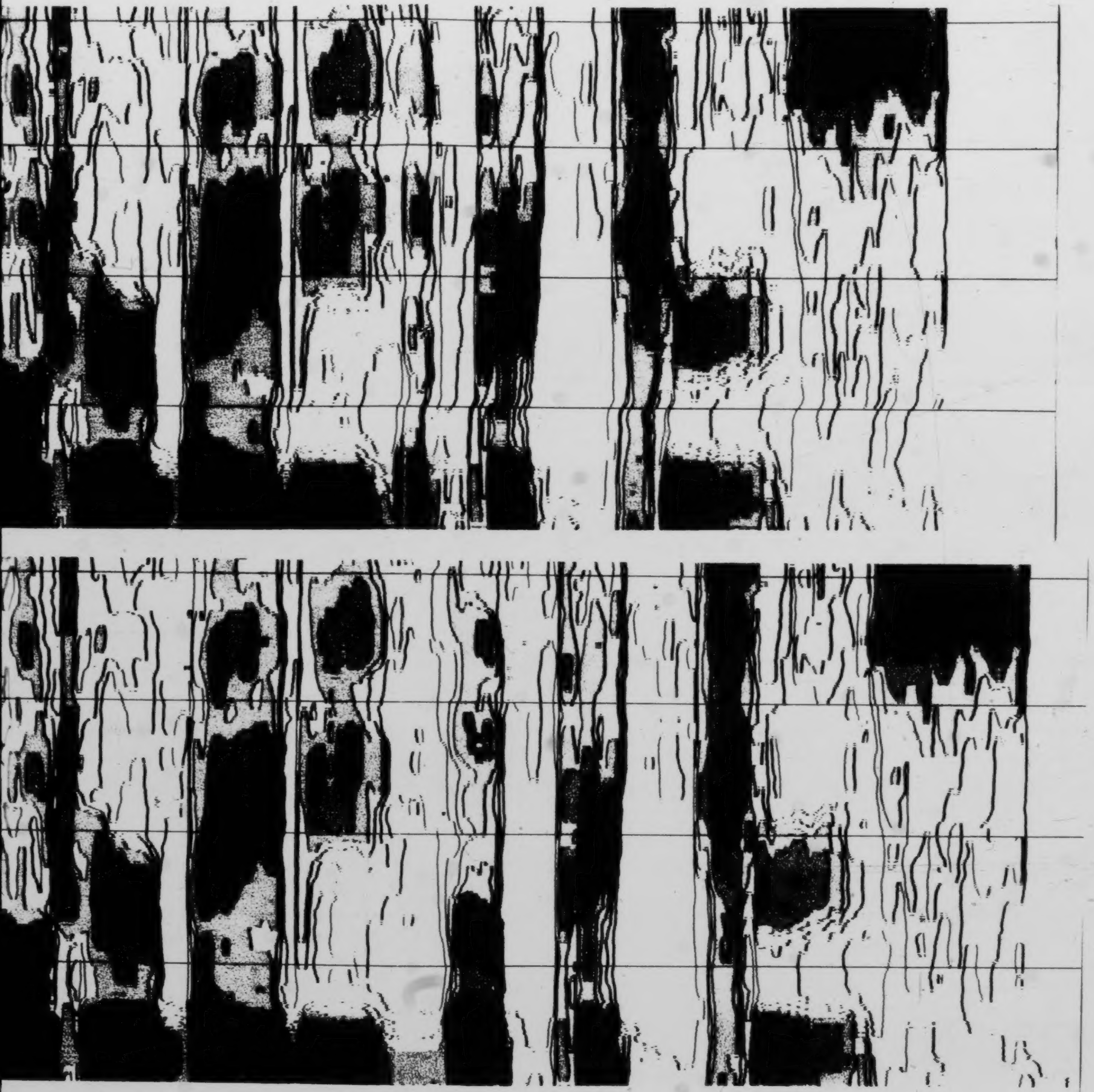
APPENDIX D

STIMULI SPECTROGRAMS



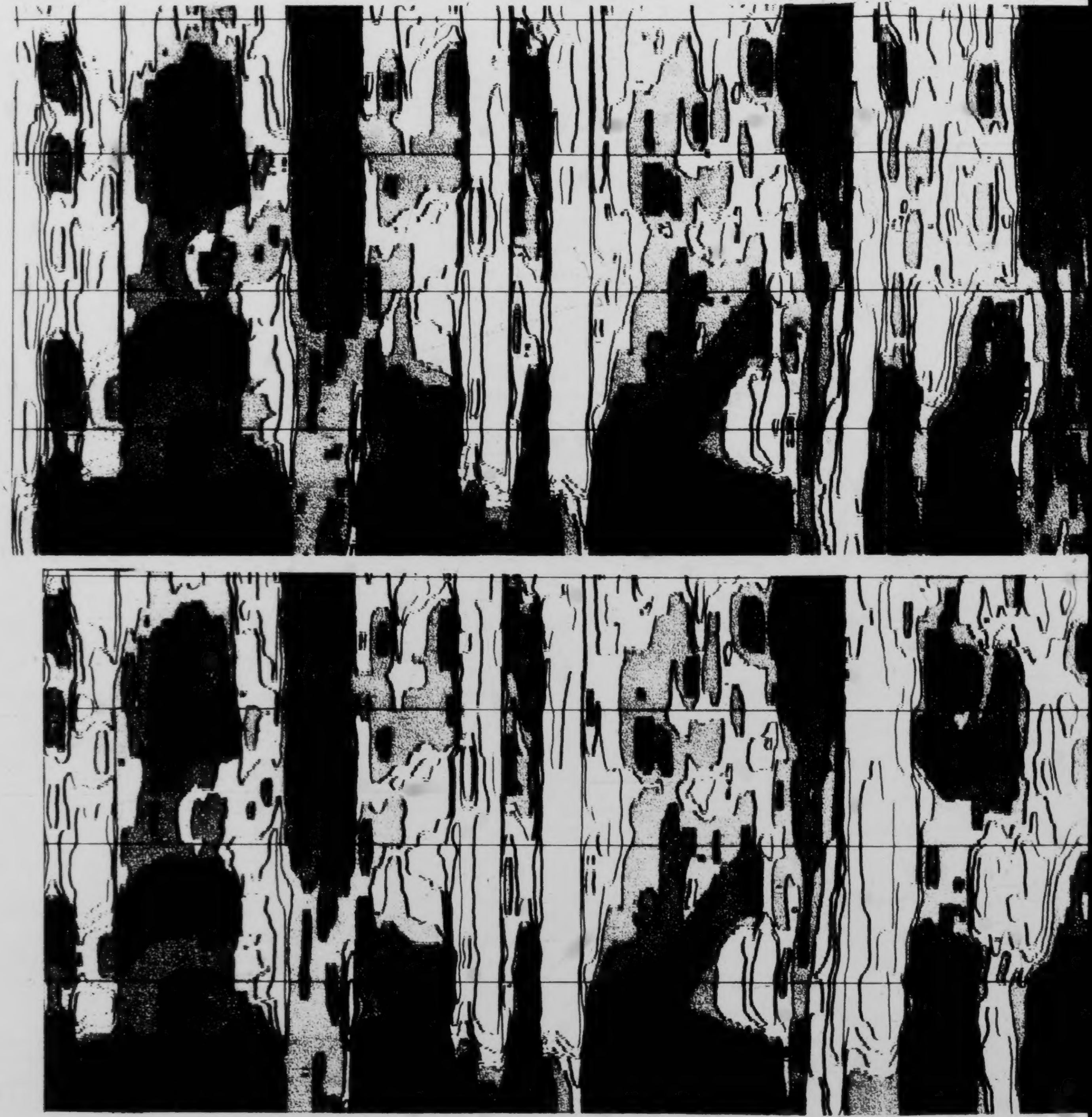
The man showed her · baby the pictures.

17



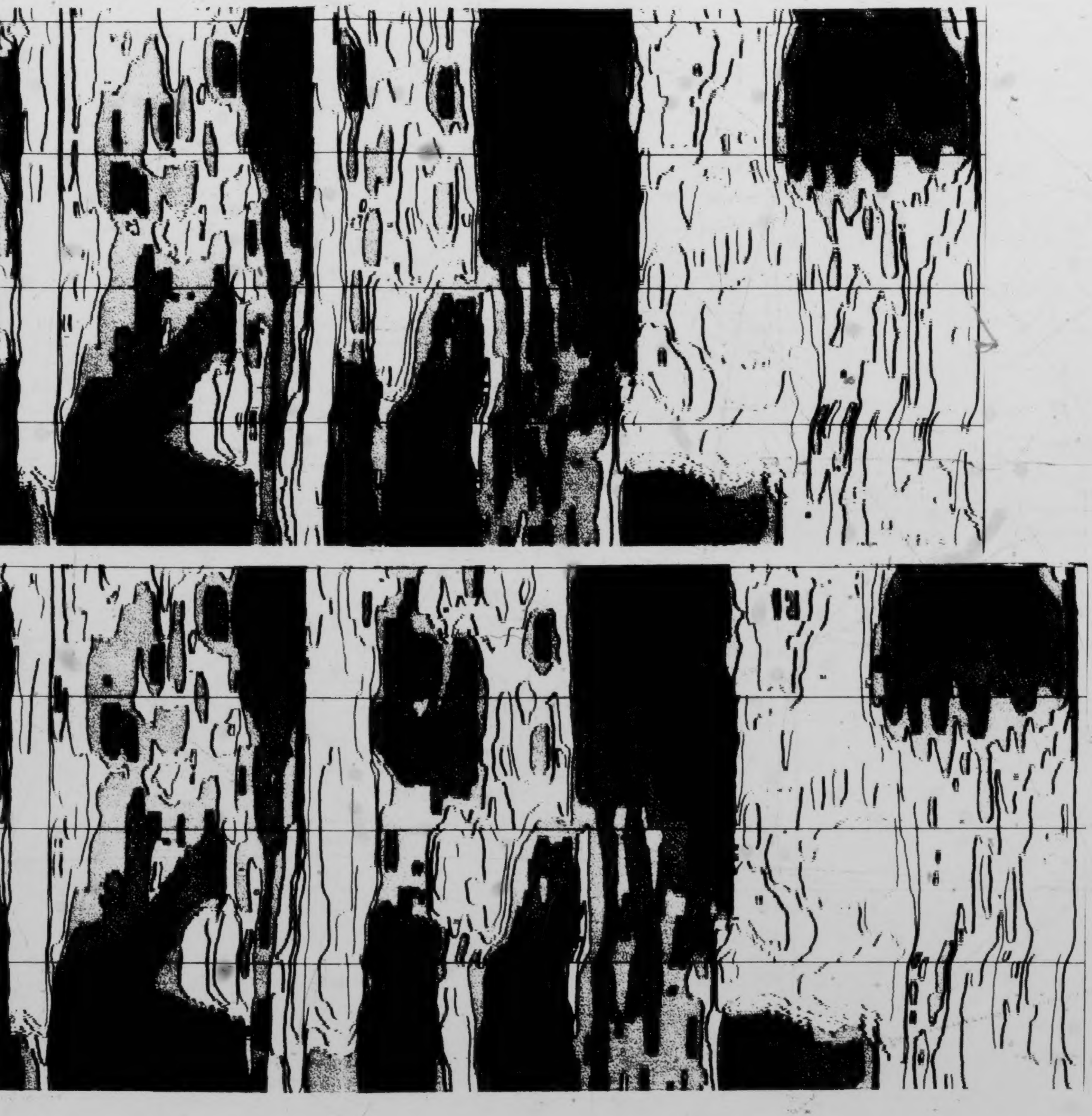
her baby the pictures.

2 of 2



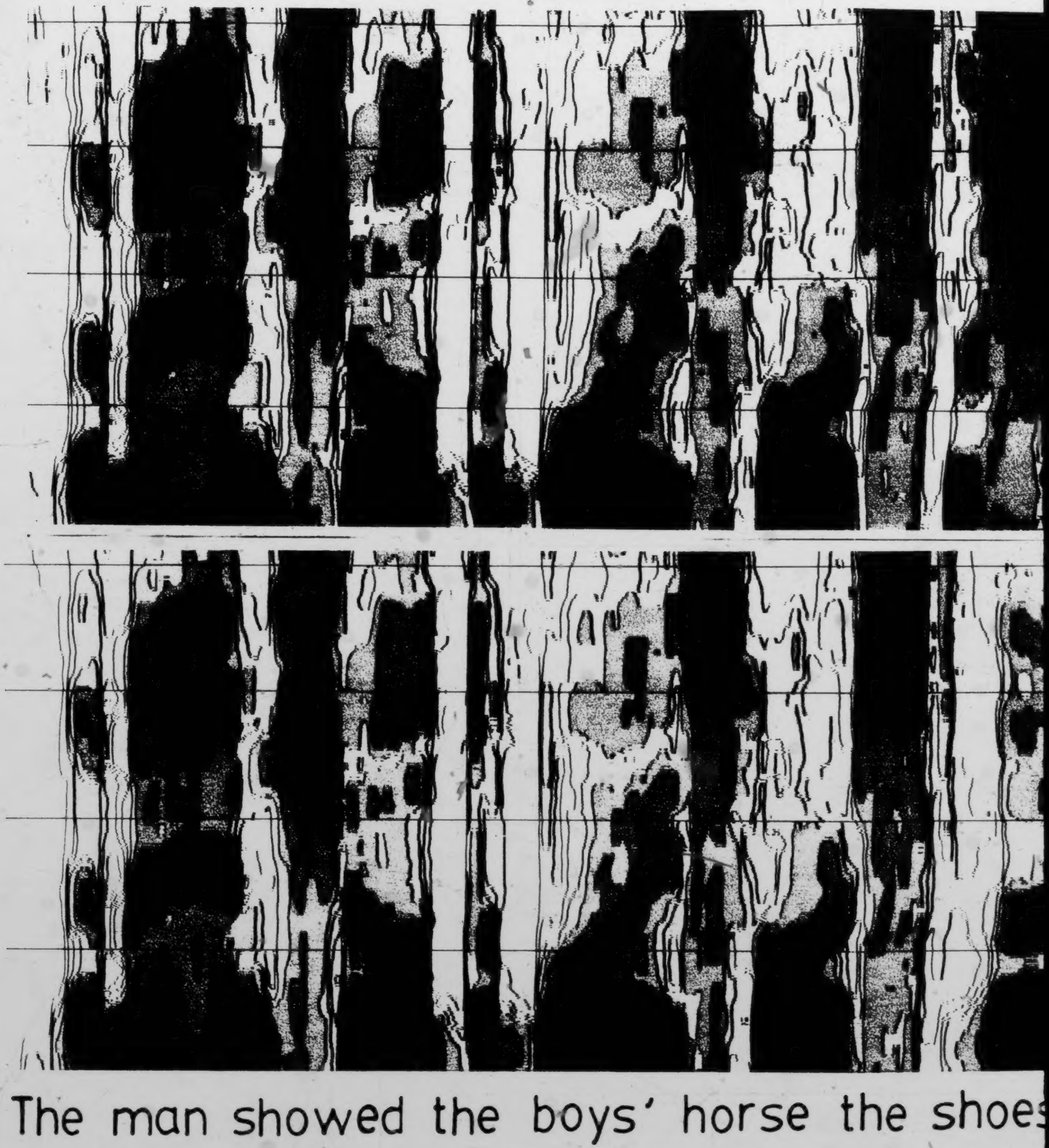
The man showed the boys the horse shoe

1 of



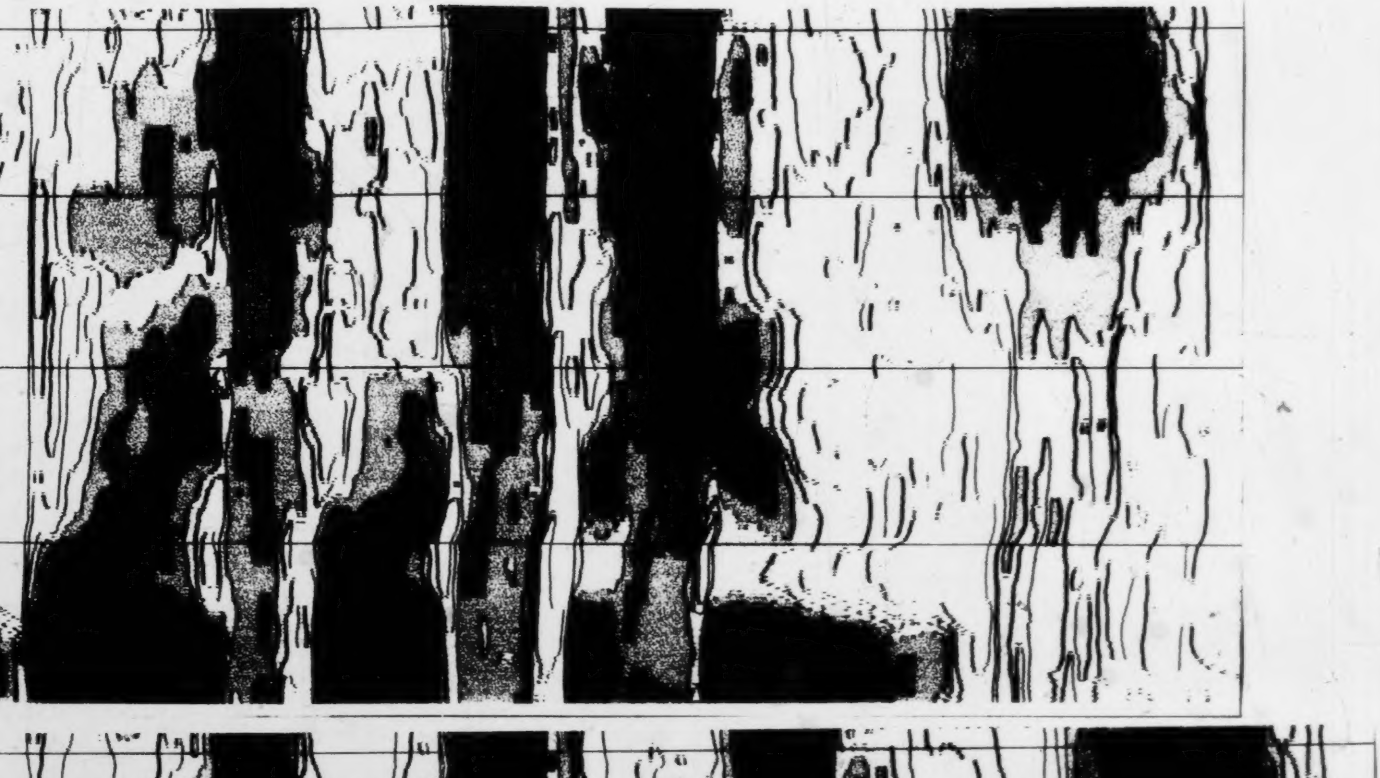
e boys the horse shoes.

2 of 2



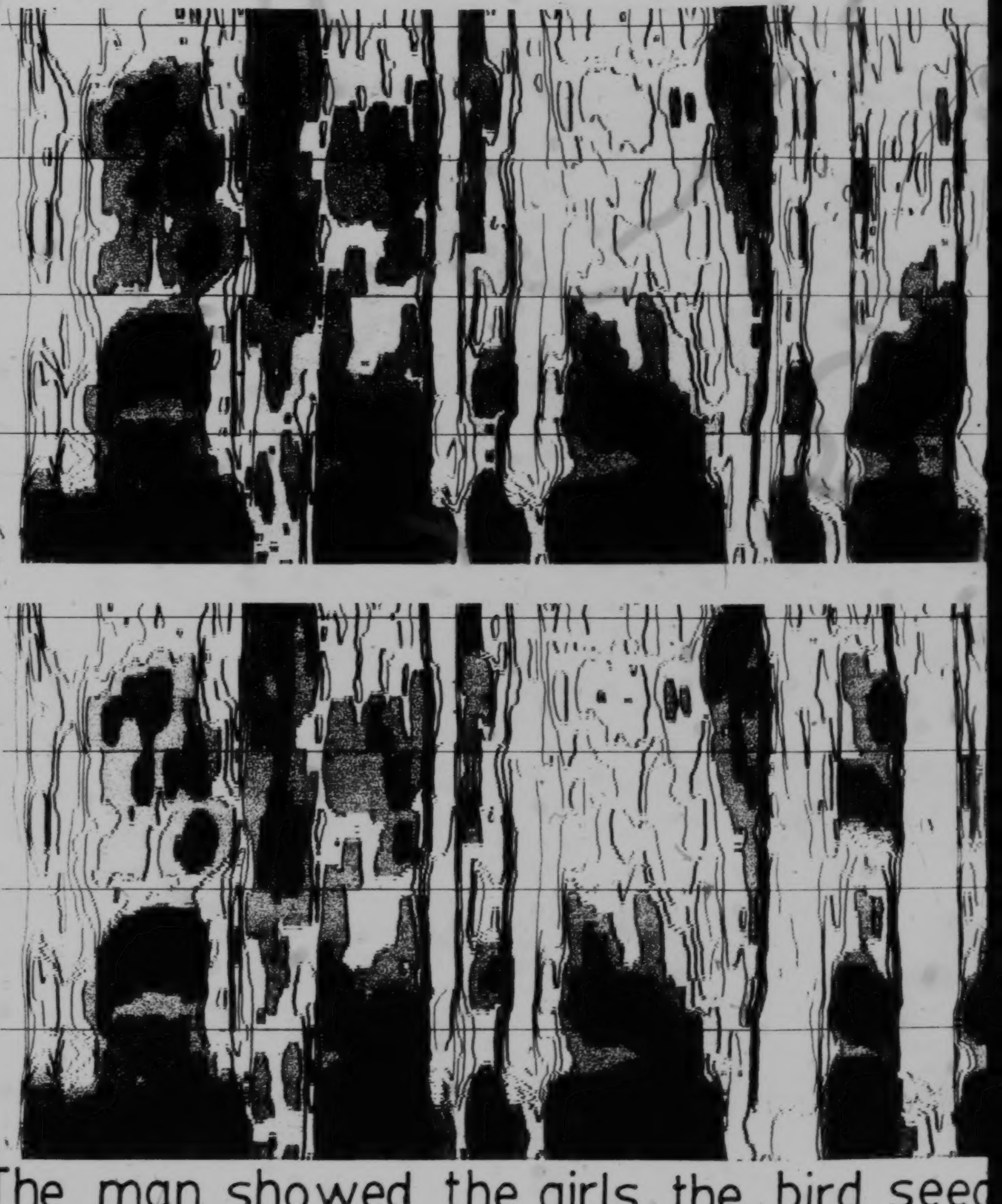
The man showed the boys' horse the shoes

17



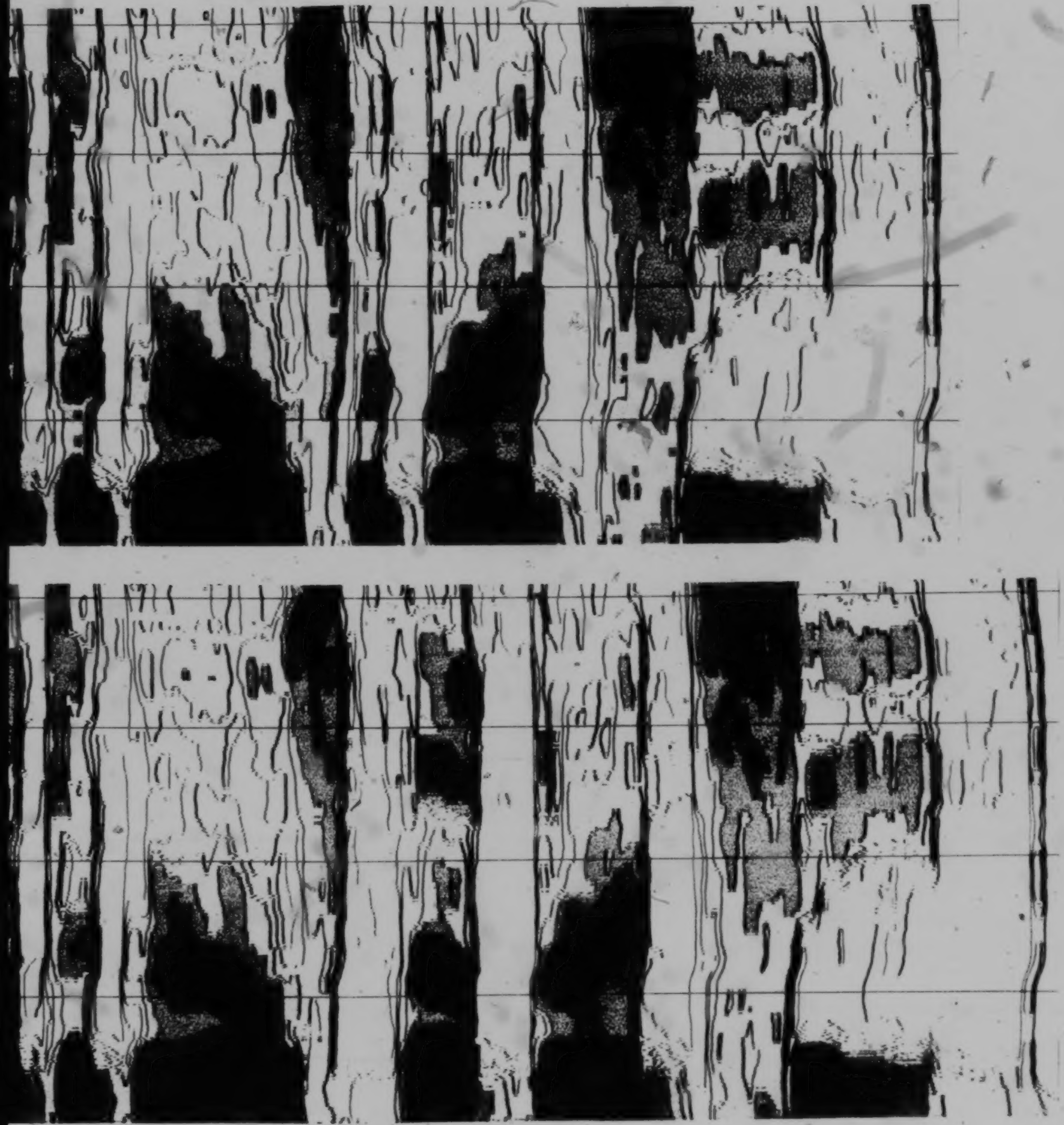
boys' horse the shoes.

2 of 2



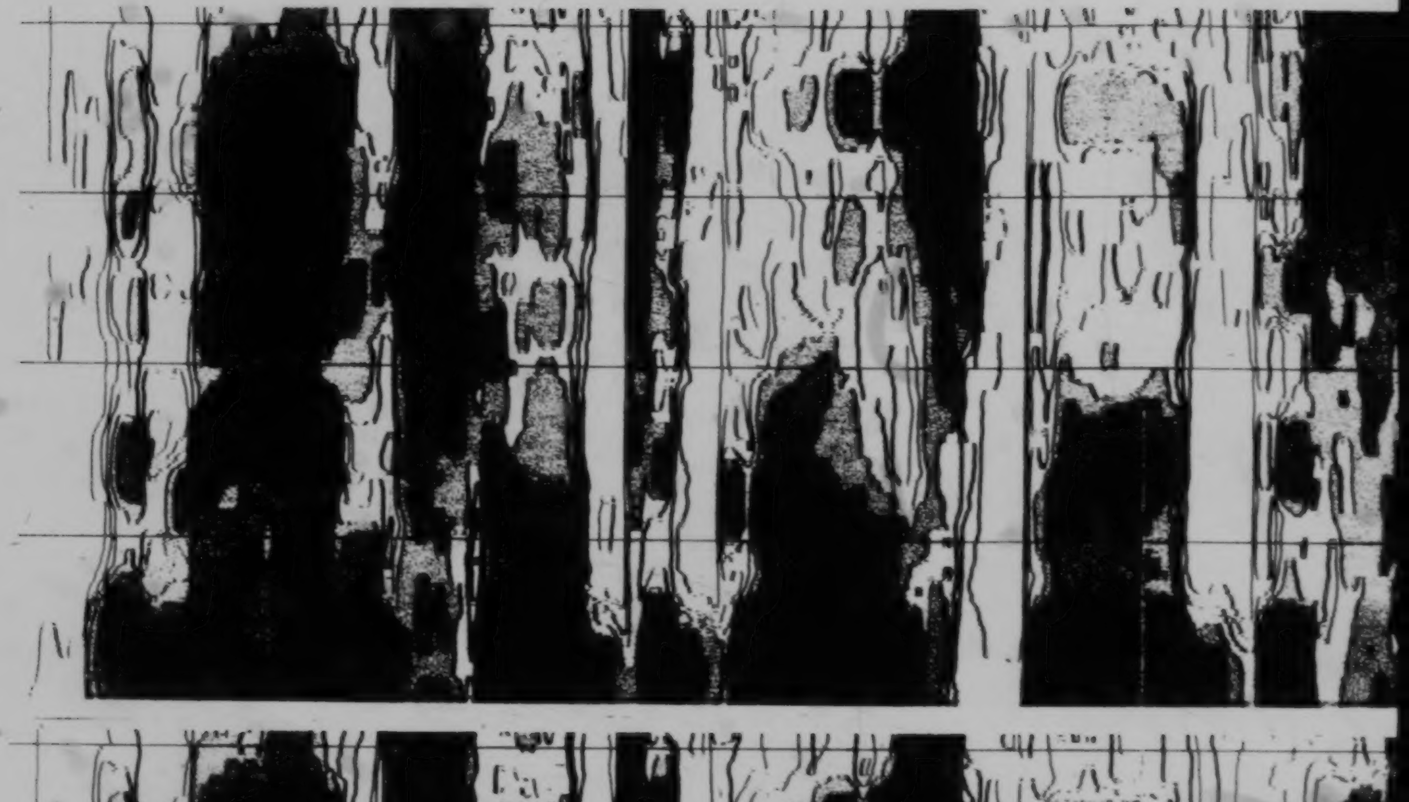
The man showed the girls the bird seed

1 of



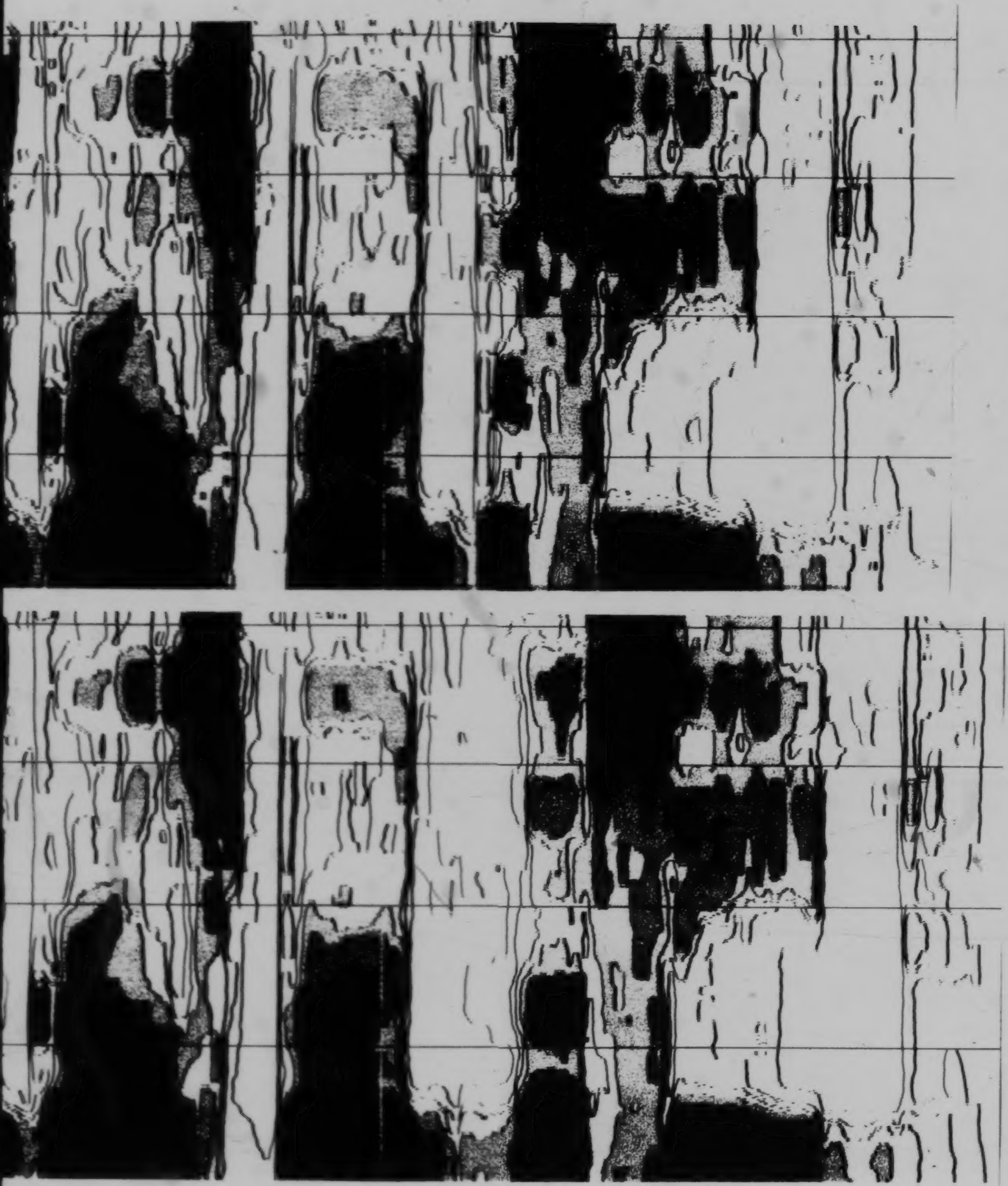
the girls the bird seed.

2 of 2



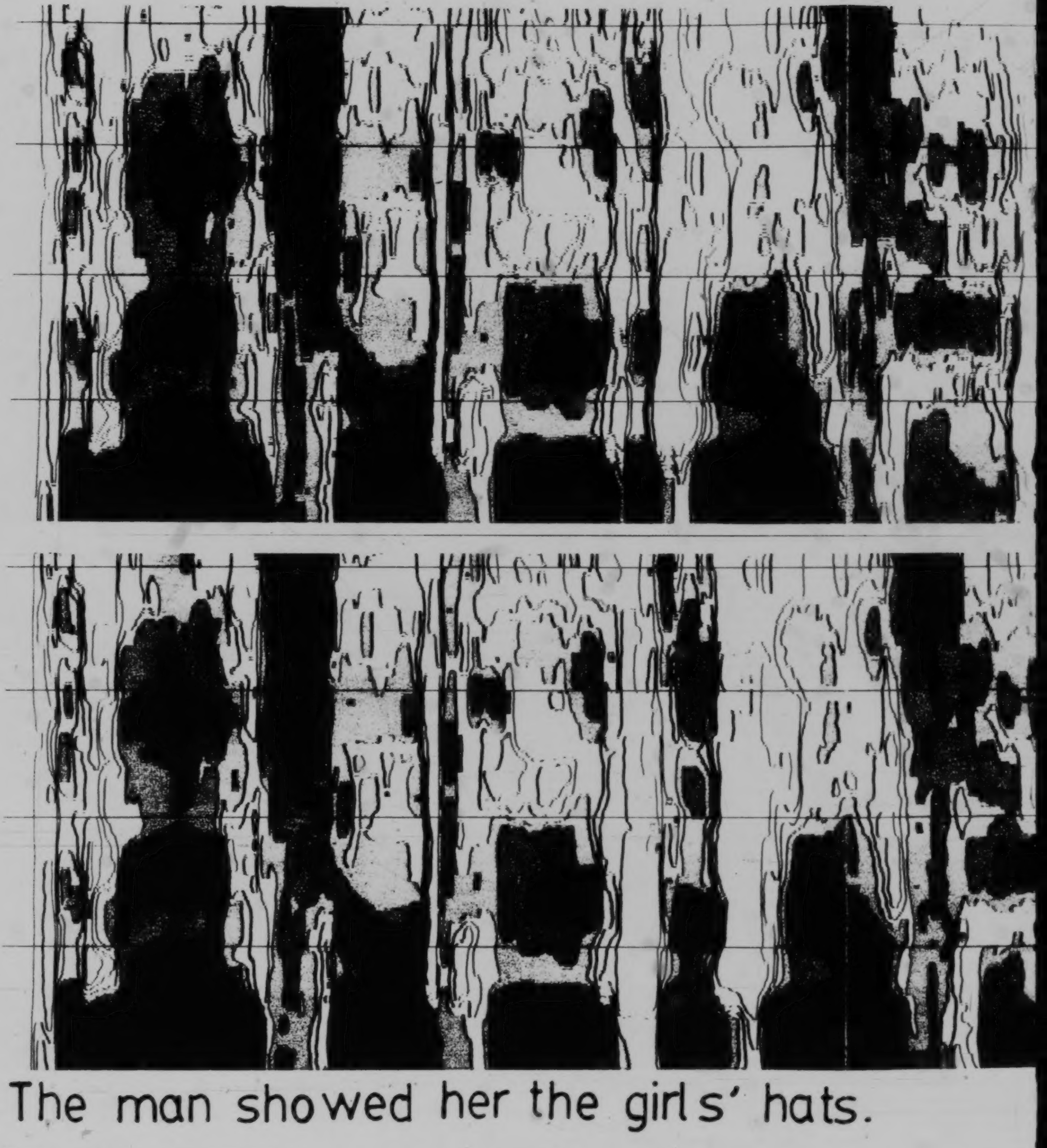
The man showed the girls' bird the seed.

7



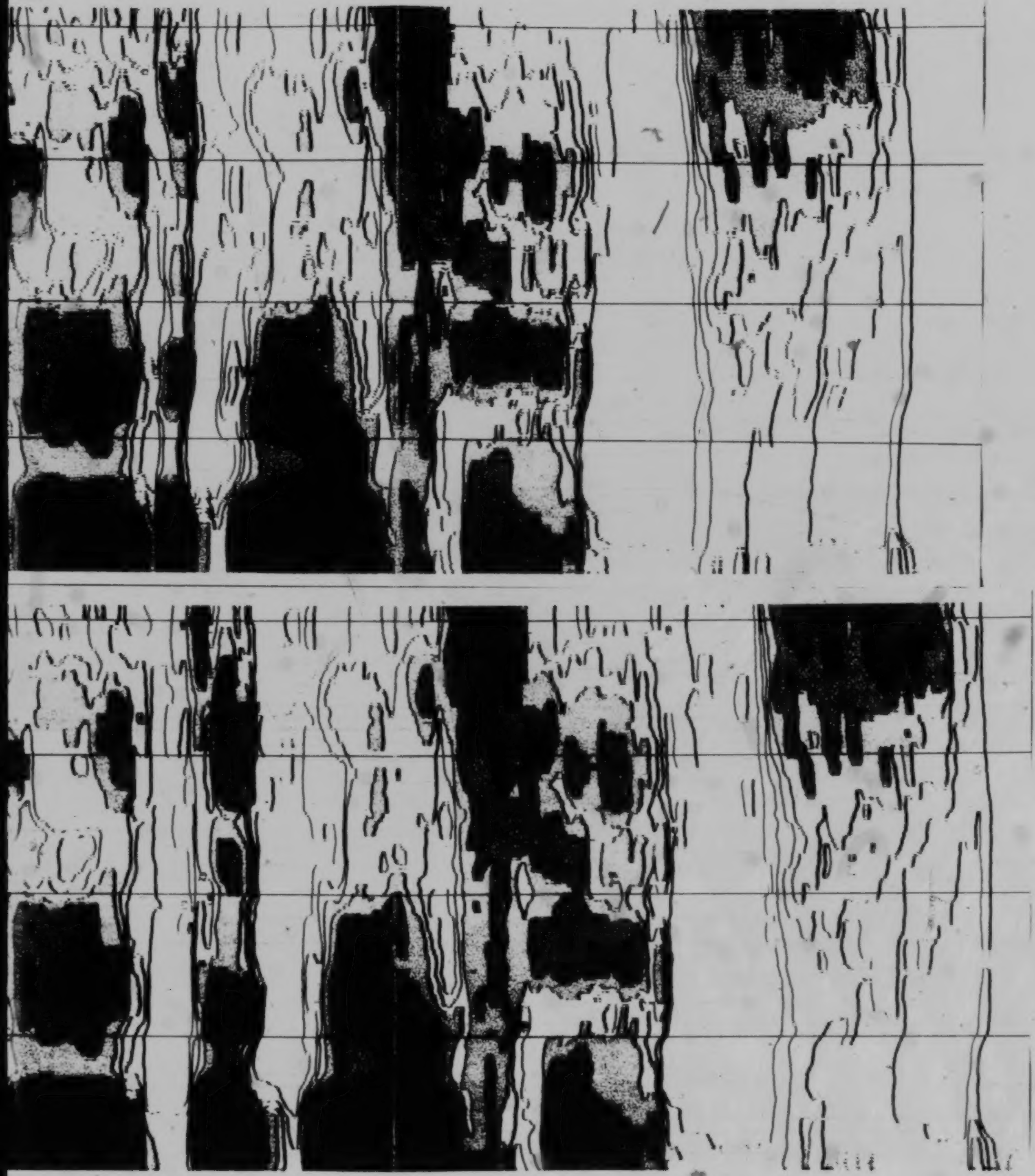
e girls' bird the seed.

2 of 2



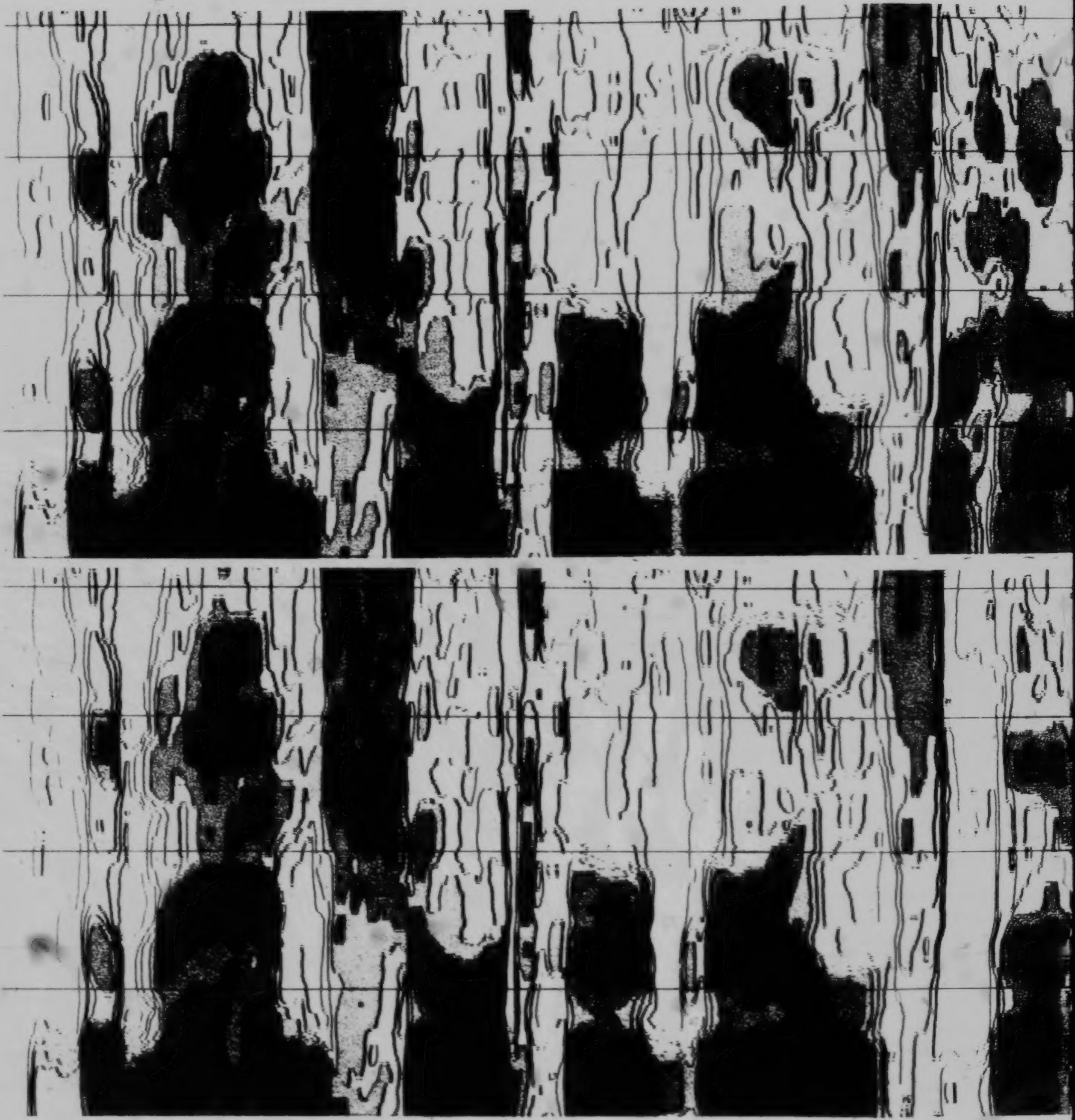
The man showed her the girls' hats.

7



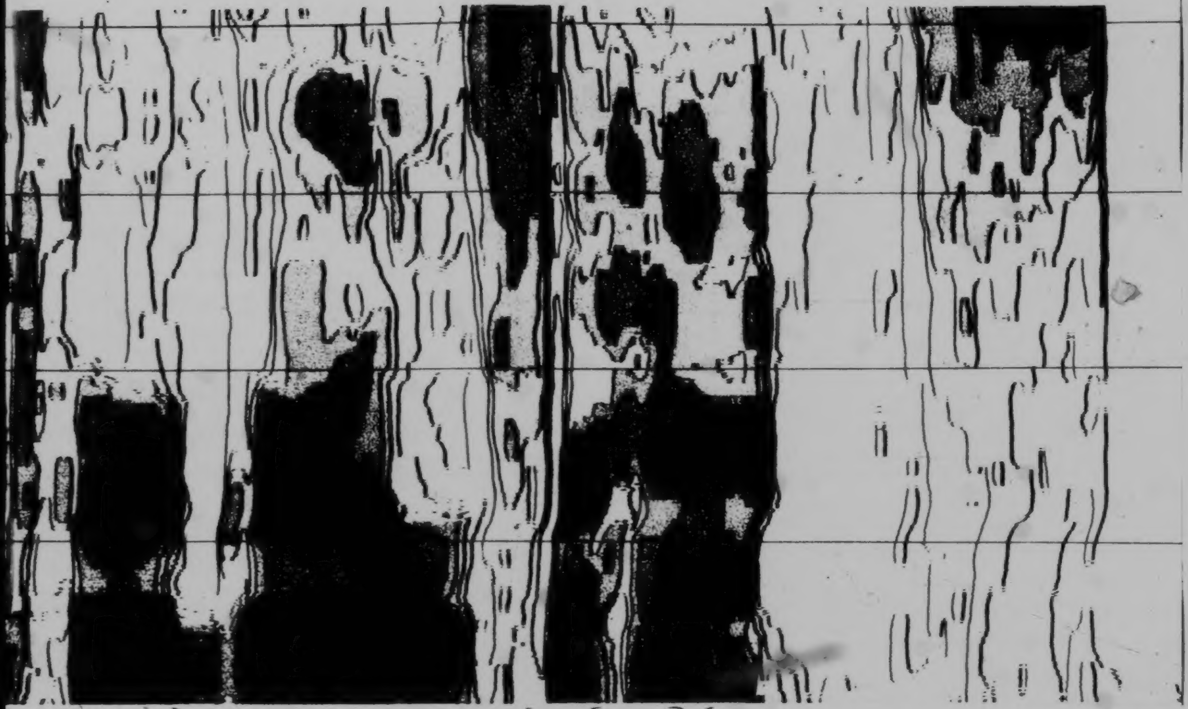
r the girls' hats.

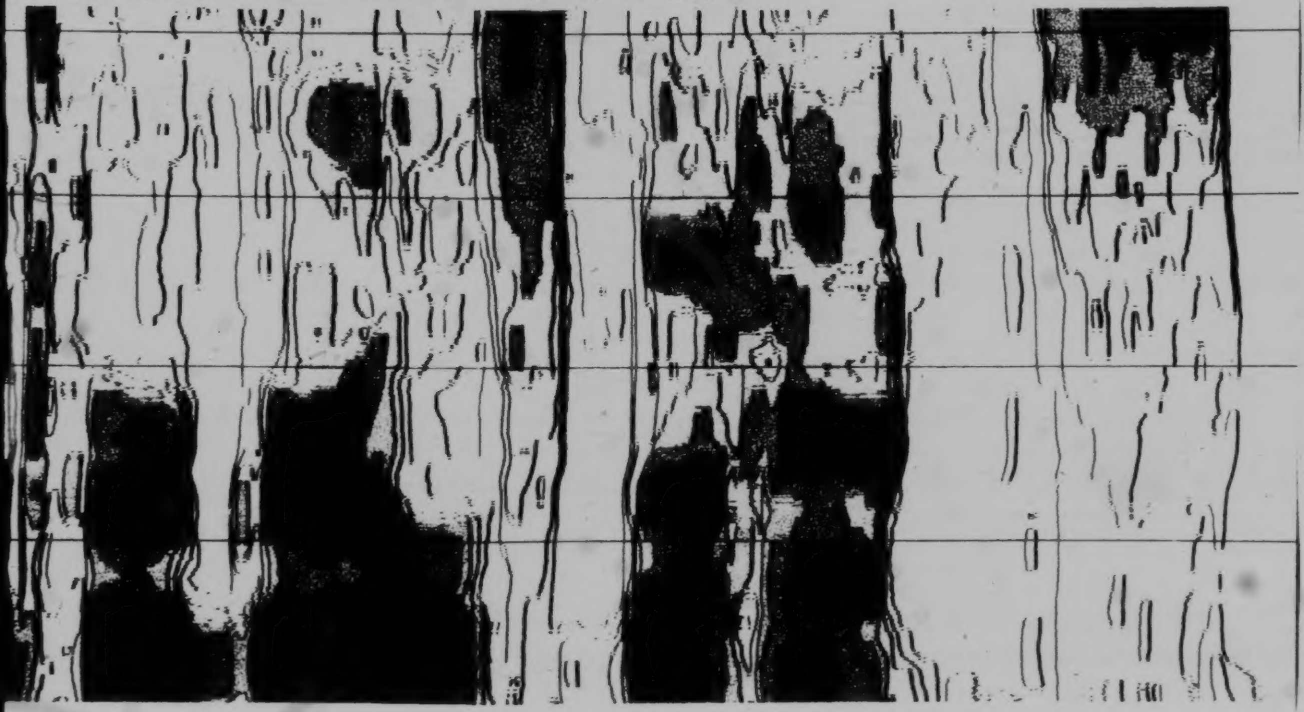
2 of 2



The man showed her girls the hats.

7



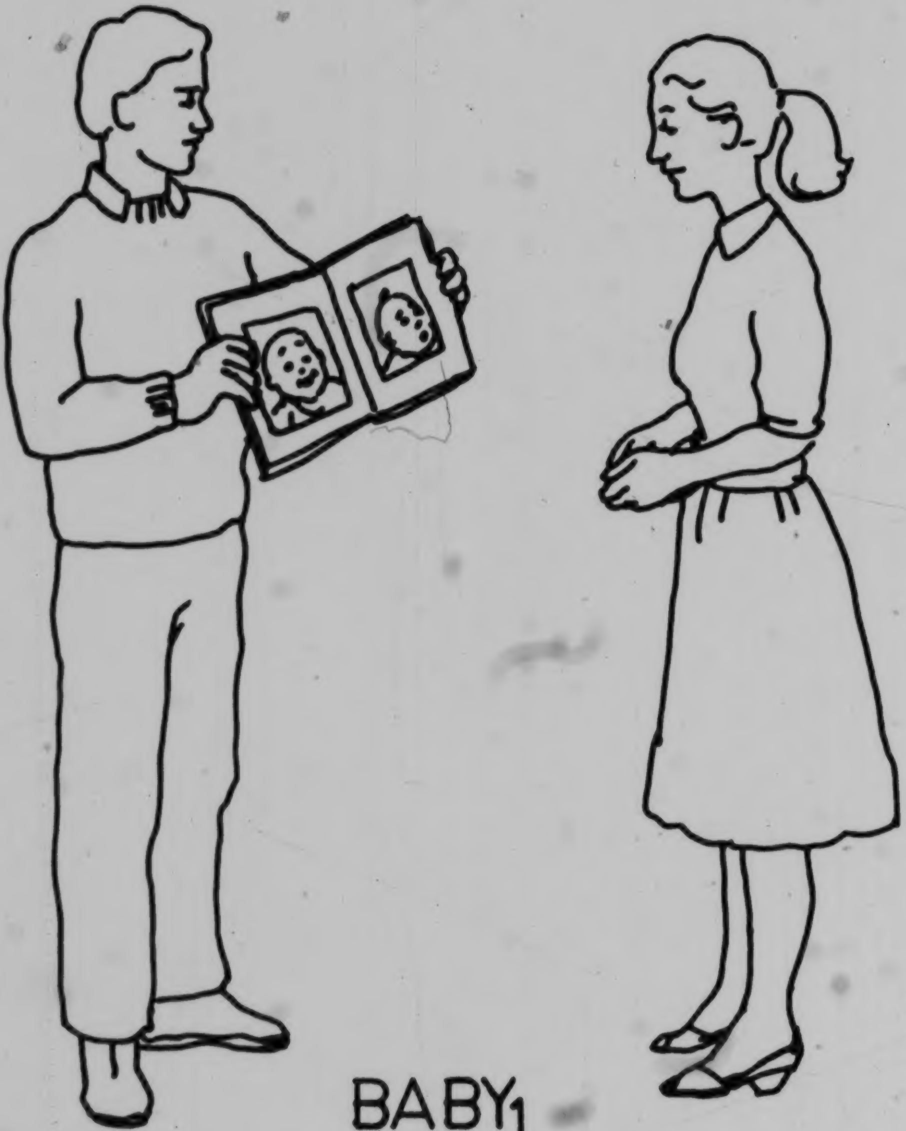


girls the hats.

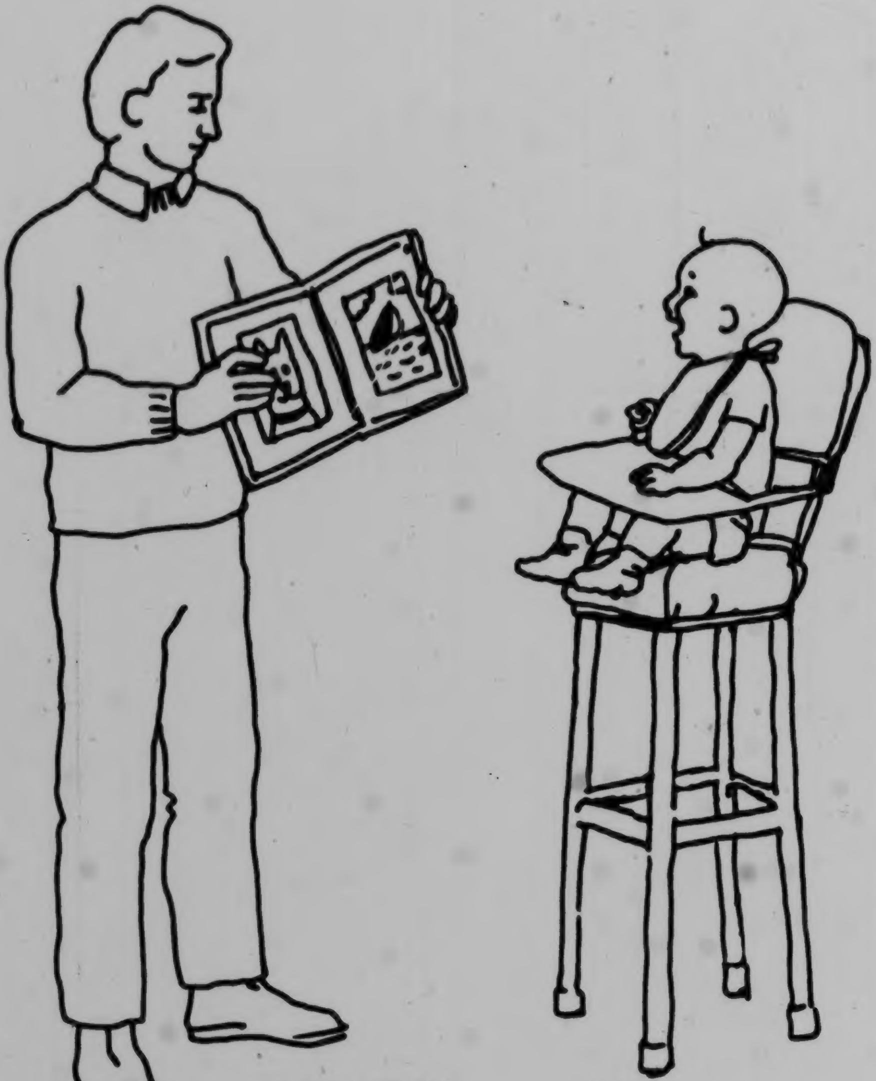
2 of 2

APPENDIX E

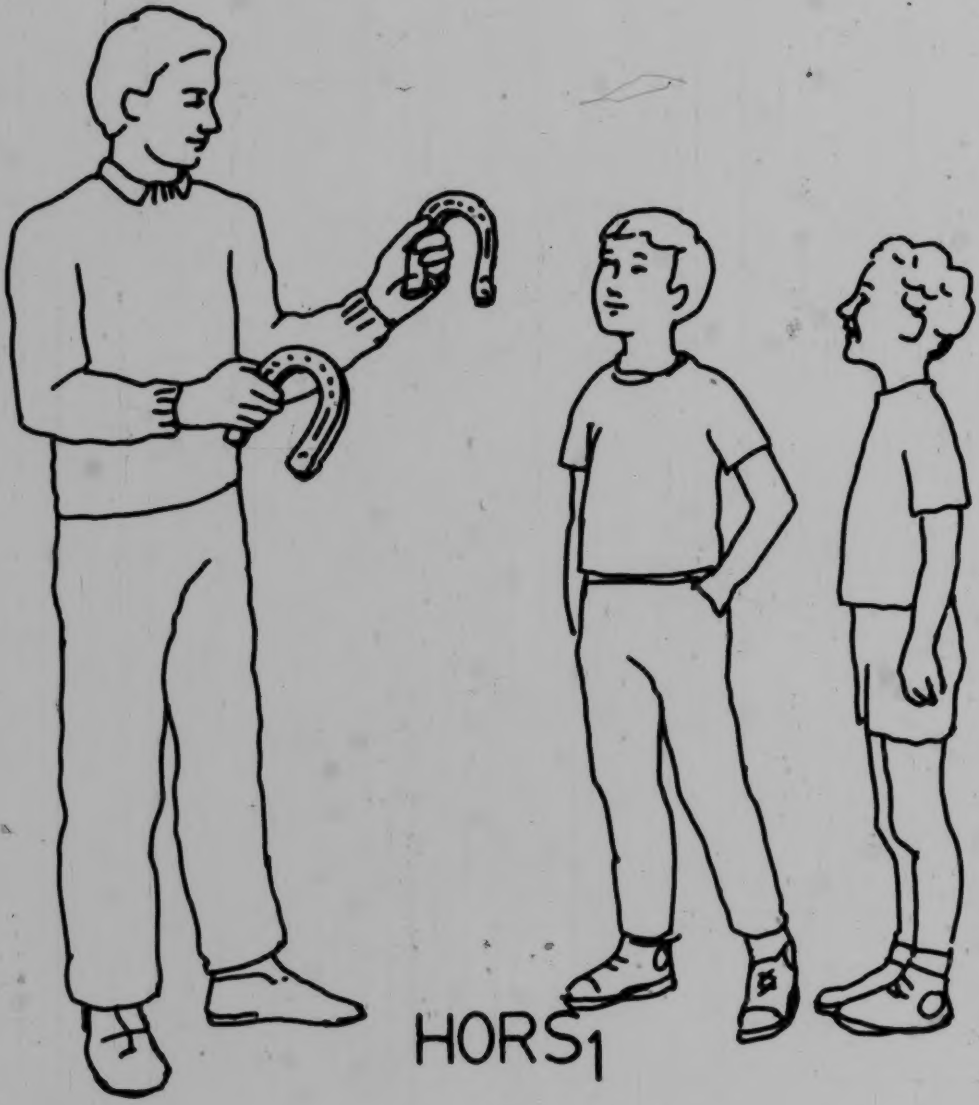
RESPONSE ITEMS



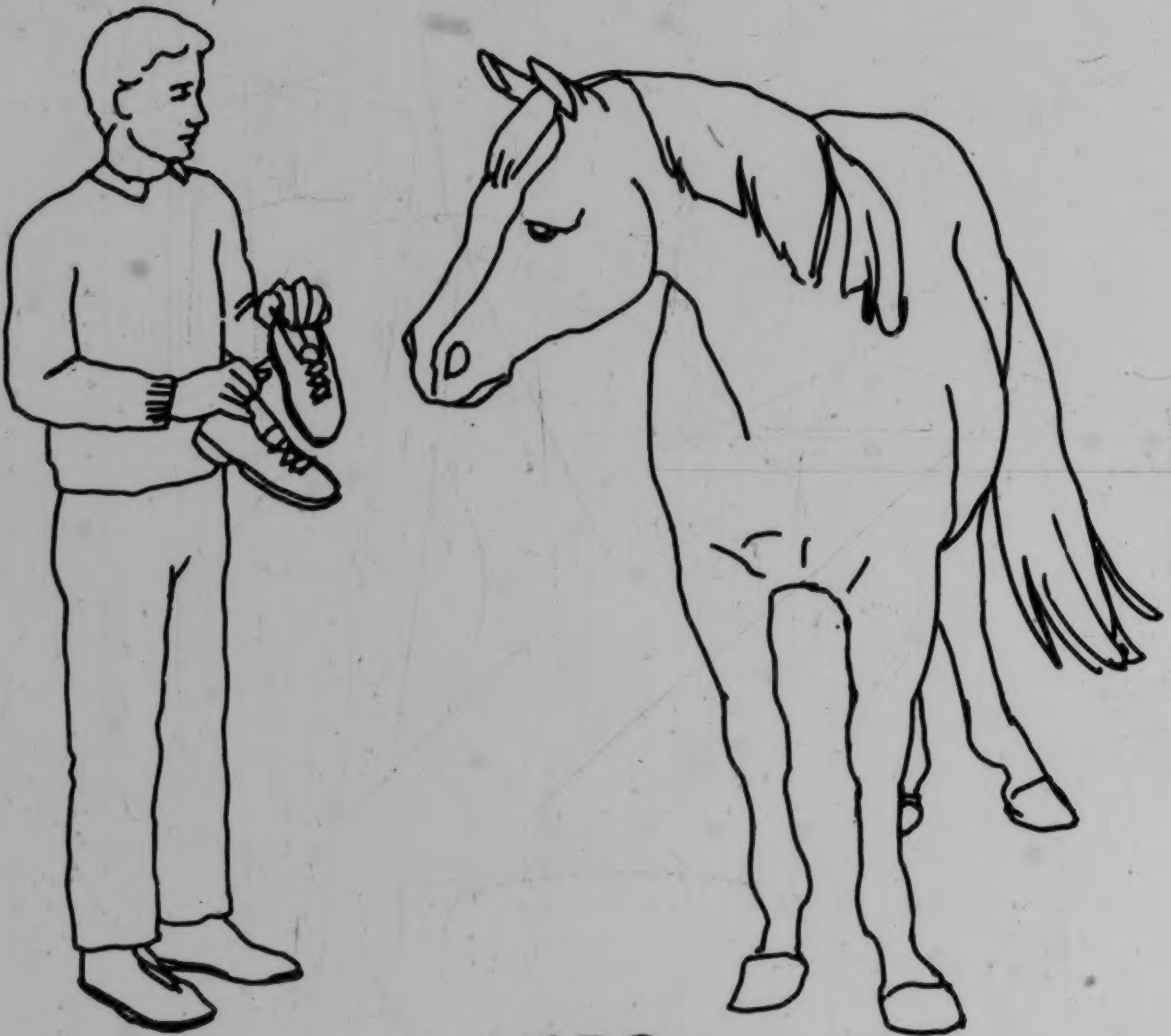
BABY$_1$



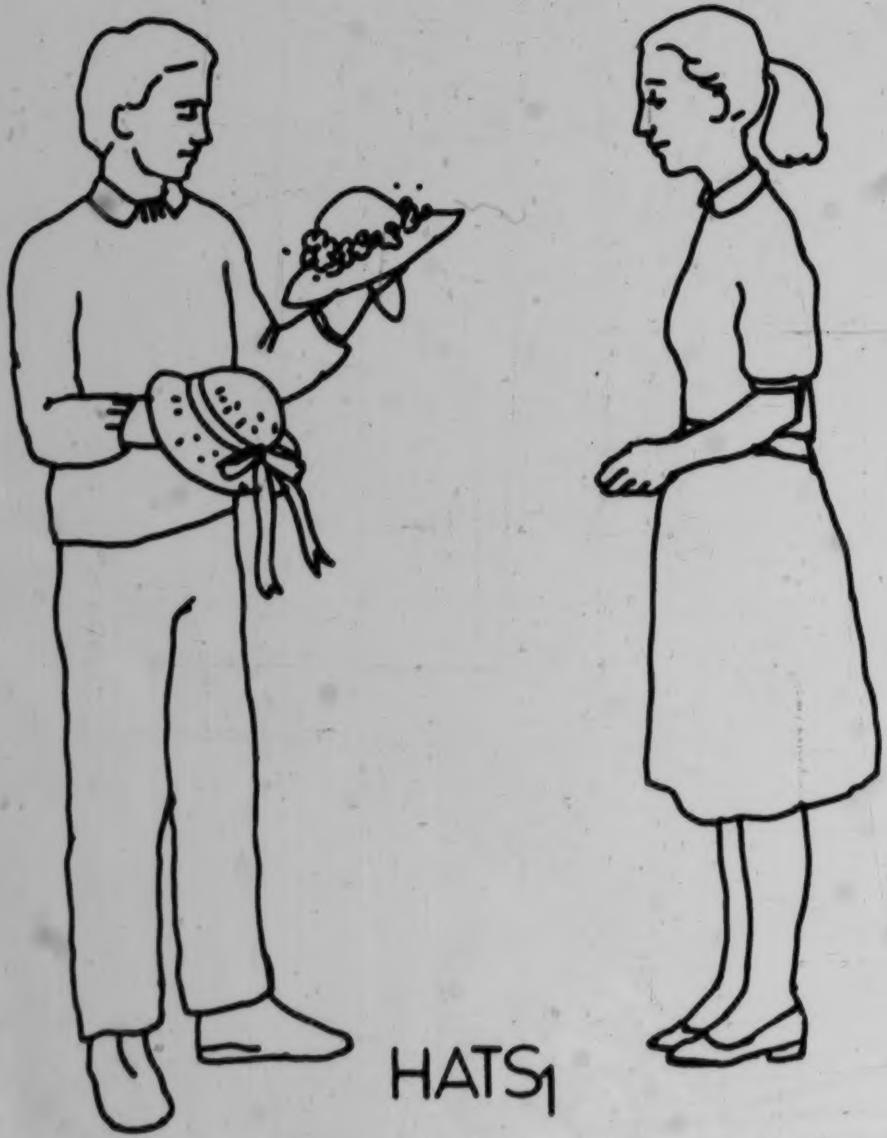
BABY$_2$



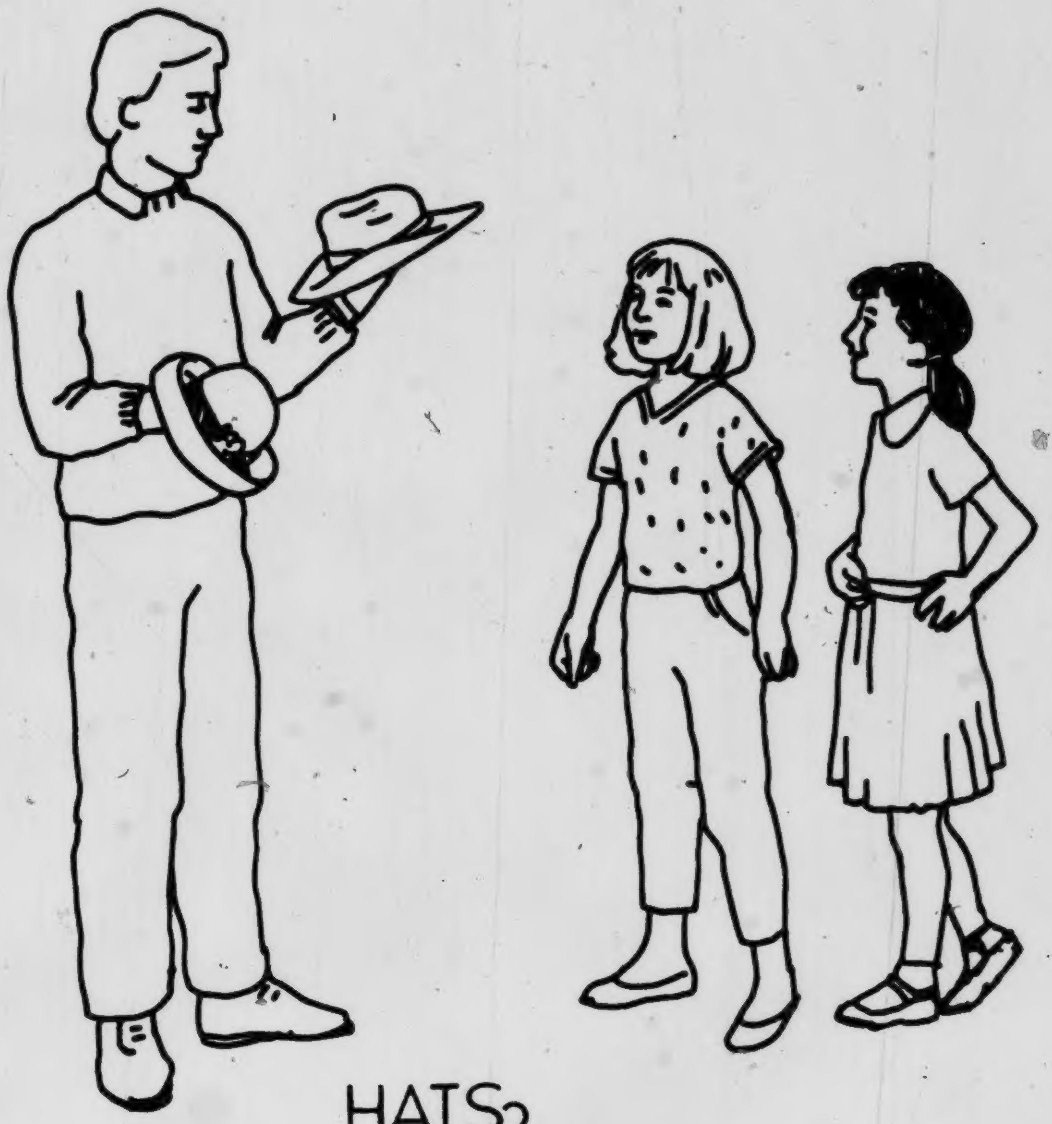
HORS1



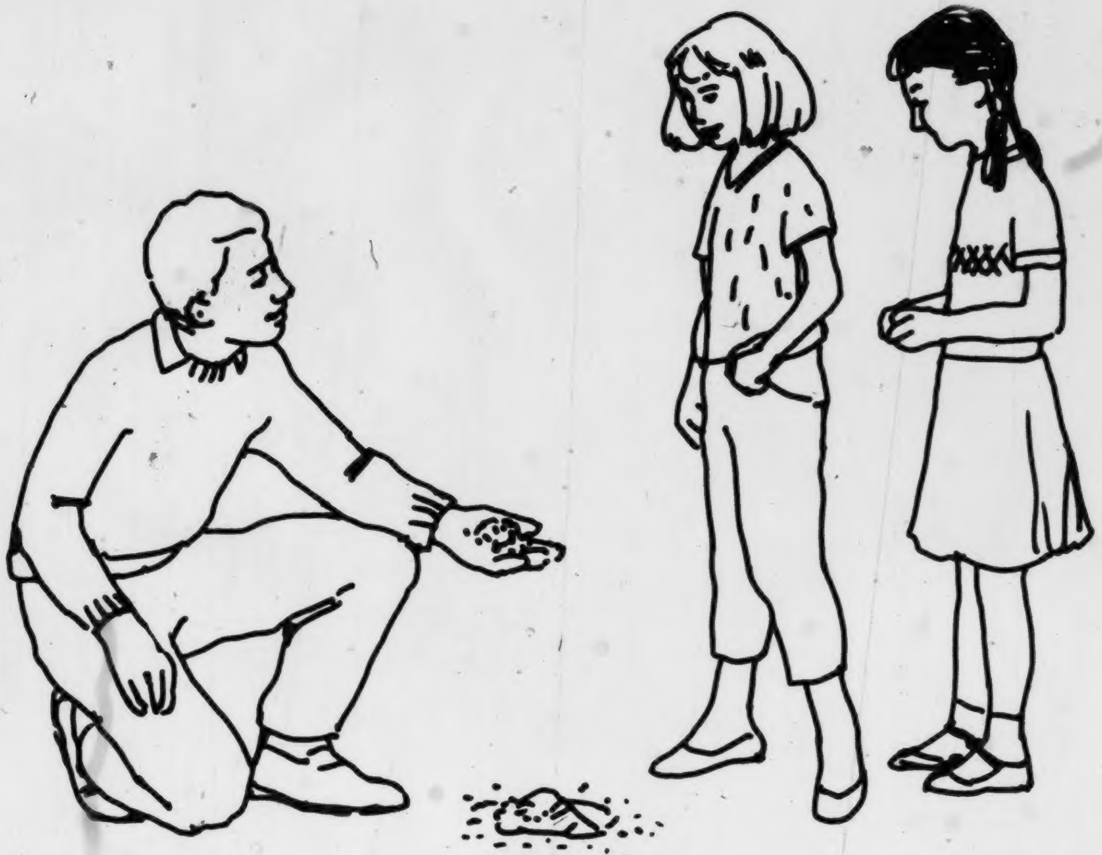
HORS$_2$



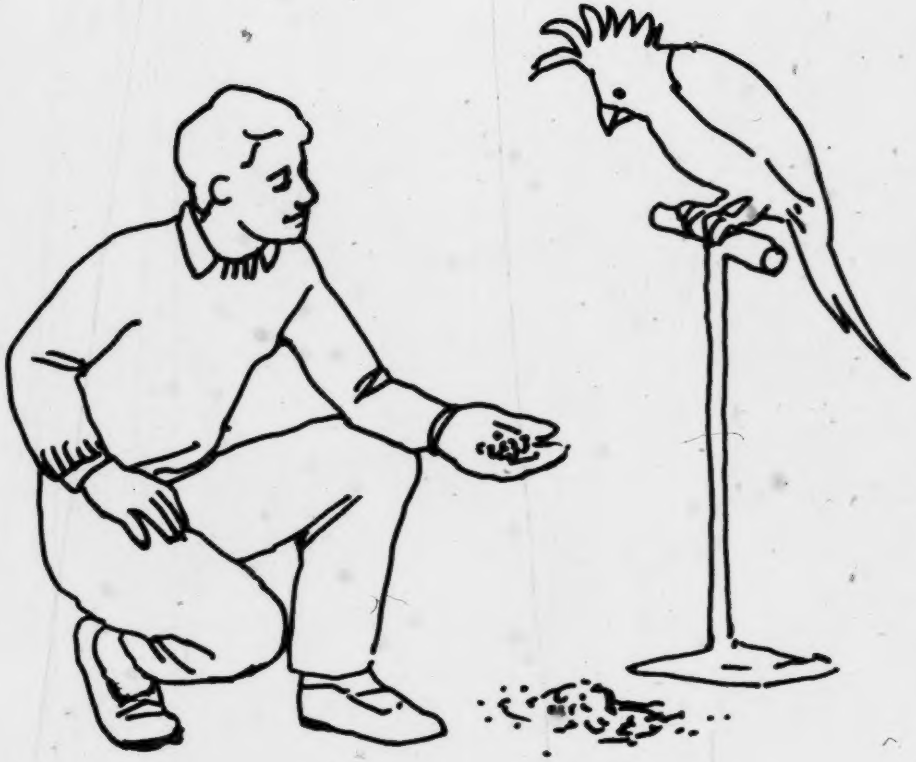
HATS$_1$



HATS$_2$



BIRD1



BIRD$_2$

APPENDIX F: *Fortran 77 programs used to collate and analyze response data*

```
MEAN
  C     A program to: read in subject responses; calculate
  C     the CORRect responses, the FUNCTion errors, and the
  C     CONTent errors for each subject and condition; find
  C     the total correct (TC), total function errors (FE),
  C     and total content errors (CE); calculate the mean
  C     correct responses (AVRC), the mean function errors
  C     (AVRFE), and the mean content errors (AVRCE) overall;
  C     find the mean correct response (AVRC1, AVRC2), the
  C     mean function errors (AVRFE1, AVRFE2), and the mean
  C     content errors (AVRCE1, AVRCE2) for the 2 conditions,
  C     where 1 = normal, and 2 = salient; and to print out
  C     the calculations.
  C
  C
  C     Variable declaration:
  C
        CHARACTER * 4 RESP(9, 2, 8)
        INTEGER C, FE, CE, I, J, K, TC, TFE, TCE
        INTEGER TC1, TFE1, TCE1, TC2, TFE2, TCE2
       -SUBJN, CONDN, SENTN
        REAL AVRC, AVRFE, AVRCE, AVRC1, AVRFE1, AVRCE1
        REAL AVRC2, AVRFE2, AVRCE2
   C
   C    Variable initialization:
   C
        SUBJN = 9
        CONDN = 2
        SENTN = 8
        C = FE = CE = TC = TFE = TCE = TC1 = TFE1 = TCE1 =
       -TC2 = TFE2 = TCE2 = 0
        I = J = K = 1
   C
   C    Print out table headings:
   C
        PRINT 1, 'SUBJECT', 'CONDITION', 'CORRECT',
       -'FUNCT ERROR(S)', 'CONT ERROR(S)'
      1 FORMAT (///, 2X, A7, 5X, A9, 5X, A7,
       -5X, A14, 5X, A13, ///)
   C
   C    Read in the responses, summing over subject (I) and
   C    condition (K):
   C
        WHILE (I.LE.SUBJN) DO
           WHILE (J.LE.CONDN) DO
              WHILE (K.LE.SENTN) DO
                 READ 2, RESP(I, J, K)
    2            FORMAT (7X, A4)
                 IF (RESP(I, J, K).EQ.'CORR') THEN DO
                    C = C + 1
           ENDIF
           IF (RESP(I, J, K).EQ.'FUNC') THEN DO
             FE = FE + 1
           ENDIF
           IF (RESP(I, J, K).EQ.'CONT') THEN DO
             CE = CE + 1
           ENDIF
           K = K + 1
         ENDWHILE
         IF (I.EQ.1) NAME = 'RS'
         IF (I.EQ.2) NAME = 'FD'
         IF (I.EQ.3) NAME = 'JC'
         IF (I.EQ.4) NAME = 'HT'
         IF (I.EQ.5) NAME = 'WP'
         IF (I.EQ.6) NAME = 'AM'
         IF (I.EQ.7) NAME = 'DS'
         IF (I.EQ.8) NAME = 'JP'
         IF (I.EQ.9) NAME = 'JF'
         IF (J.EQ.1) THEN DO
           TC1 = TC1 + C
           TFE1 = TFE1 + FE
           TCE1 = TCE1 + CE
         ENDIF
         IF (J.EQ.2) THEN DO
           TC2 = TC2 + C
           TFE2 = TFE2 + FE
           TCE2 = TCE2 + CE
         ENDIF
  C
  C  Print out response sums by condition:
  C
         PRINT 3, I, J, C, FE, CE
    3    FORMAT (5X, A2, 7X, A7, 9X, I1, 14X, I1, 19X,
I1)
    -    I1, 19X, I1)
  C
  C  Sum the responses overall, and initialize array
  C  variables:
  C
         TC = TC +   C
         TFE = TFE + FE
         TCE = TCE + CE
           C = FE = CE = 0
         K = 1
         J = J + 1
       ENDWHILE
       J = 1
       I = I + 1
     ENDWHILE
  C
  C  Print overall totals:
  C
     PRINT 4, 'A total of', TC, 'responses were correct.'
     PRINT 4, 'A total of', TFE,
     -'function errors were made.'
      PRINT 4, 'A total of', TCE,
     -'content errors were made.'
    4 FORMAT (//, 5X, A10, 2X, I2, 2X, A26)
C
C     Calculate overall means:
C
      AVRC = FLOAT (TC) / FLOAT (SUBJN)
      AVRFE = FLOAT (TFE) / FLOAT (SUBJN)
      AVRCE = FLOAT (TCE) / FLOAT (SUBJN)
C
C     Print overall means (to 3 decimal places):
C
      PRINT 5, 'The average correct response was:', AVRC
      PRINT 5,
     -'The average number of function errors was:', AVFE
      PRINT 5,
     -'The average number of content errors was: ', AVRCE
    5 FORMAT (//, 5X, A42, 19X, F7.3)
C
C     Calculate means by condition:
C
      AVRC1 = FLOAT (TC1) / FLOAT (SUBJN)
      AVRFE1 = FLOAT (TFE1) / FLOAT (SUBJN)
      AVRCE1 = FLOAT (TCE1) / FLOAT (SUBJN)
      AVRC2 = FLOAT (TC2) / FLOAT (SUBJN)
      AVRFE2 = FLOAT (TFE2) / FLOAT (SUBJN)
      AVRCE2 = FLOAT (TCE2) / FLOAT (SUBJN)
C
C     Print means by condition (to 3 decimal places):
C
      PRINT 6, 'The average correct response in
     -condition 1 was:', AVRC1
      PRINT 7, 'The average number of',
     -'function errors in condition 1 was:', AVRFE1
      PRINT 7, 'The average number of',
     -'content errors in condition 1 was: ', AVRCE1
      PRINT 6, 'The average correct response in
     -condition 2 was:', AVRC2
      PRINT 7, 'The average number of',
     -'function errors in condition 2 was:', AVRFE2
      PRINT 7, 'The average number of',
     -'content errors in condition 1 was: ', AVRCE2
    6 FORMAT (//, 5X, A48, 13X, F6.3)
    7 FORMAT (//, 5X, A21, 1X, A35, 4X, F6.3)
      STOP
      END
/EXECUTE
/END
```

RANDTTEST[24]

```
/COMPILE LIST NOEXT T=10
C   A program to read in 2 DATA-samples of measures,
C   assign INDEXes to each measure, OBTain a t-statistic
C   testing the samples, systematically permute
C   the measures and the indices, finding a t-statistic
C   for each permutation, and return a one-tailed
C   PROBability as a function of the proportion of
C   of permutations that produce t-statistics close
C   to the original value.
C
C
C   Variable declaration:
C
      INTEGER N1, N2, NN, TOTAL, DATA(18), INDEX(18),
     -SUM, I
      INTEGER TEST, NPERM, OBT, NGE
      REAL PROB
C
C   Variable initialization:
C
      N1 = 9
      N2 = 9
      NN = N1 + N2
      TOTAL = 0
C
C   Read in data, calculate SUM, TOTAL:
C
      DO 1 I = 1, NN
         READ 10, DATA(I)
10       FORMAT (2X,I1)
         TOTAL = TOTAL + DATA(I)
         IF (I.EQ.N1) SUM = TOTAL
C
C   Assign indices, permute, calculate t's:
C
  1   INDEX(I) = I
```

---

[24] This program depends on systematic permutation of cases and assigned indices, and the number of permutations escalates very quickly with the addition of new cases (the formula is (N1·N2)!/(N1!·N2!)). Consequently, the program can burn a good deal of CPU time, and can prove relatively expensive to run. If you use the *WATFIV compiler, any more than 16 cases (total) will exceed the built-in job time limits. To get around this problem a T=x (where x is an upper bound for CPU time in seconds) must be added to the /COMPILE card, as above. Eighteen cases will take about 8 seconds, 20 cases about 30 seconds, and any more than 20, unless your budget is high, should be analyzed with a random data permutation program (see Edgington [1980:84]). Notice that the arrays DATA and INDEX must be initialized to NN, and that N1 and N2 must be initialized to sample sizes (which need not be equal).

```
      OBT = SUM
      NPERM = NGE = 1
    2 I = N1
    3 IF (INDEX(I).EQ.NN) GO TO 6
      INDEX(I) = INDEX(I) + 1
      IF (I.EQ.N1) GO TO 4
      I = I + 1
      INDEX(I) = INDEX(I - 1)
      GO TO 3
    4 NPERM = NPERM + 1
      SUM = 0
      DO 5 I = 1, N1
    5 SUM = SUM + DATA(INDEX(I))
      TEST = SUM
      IF (TEST.GE.OBT) NGE = NGE + 1
      GO TO 2
    6 I = I - 1
      IF (I.NE.0) GO TO 3
C
C  Calculate PROB, print PROB:
C
      PROB = FLOAT(NGE) / FLOAT(NPERM)
      PRINT, PROB
      STOP
      END
/EXECUTE
/END
```